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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

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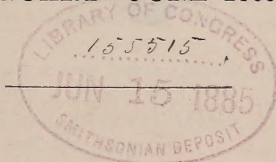
AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. l. Not.*

VOL. XIX.—FIFTH SERIES.

JANUARY—JUNE 1885.



LONDON:

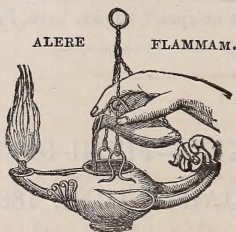
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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quaestionem, quaestio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phoebus ferrugine condant,
Quid toties diros cogat flagrare cometas,
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium



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[FIFTH SERIES.]

JANUARY 1885.

- I. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part IX. By C. R. ALDER WRIGHT, D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School*.

On Voltaic and Thermovoltaic Constants.

164. **T**HE experiments described in Part VIII.† show that the difference in E.M.F. between two two-fluid cells containing metallic salts in solution, and alike in all respects save that one of the fluids is of different degrees of concentration in the two cells respectively, can be readily obtained by opposing the two cells to one another, and measuring the current set up through a total resistance large enough to reduce the current to such an extent that its density, in reference to the plate-surfaces in the cells, falls below a certain limiting value variable with the details of the construction of the cells; and, further, that the same value is obtained by means of "diffusion-cells," consisting of two vessels containing respectively the two solutions of different degrees of concentration but containing the same salt, united by a siphon-tube, or constituting a "gravity" arrangement. If a be the increment in E.M.F. thus measured, due to a given increase in the strength of the fluid surrounding the plate acquiring the higher potential, and b be the corresponding

* Communicated by the Physical Society. Read November 8, 1884.

† Phil. Mag. vol. xvii. pp. 282, 377.

decrement* due to a given increase in strength of the other fluid, then E and e , the electromotive forces of the cell when containing the two stronger and the two weaker fluids respectively, are related so that, within experimental-error limits,

$$E = e + a - b.$$

It follows from this fact, and from the circumstance that in many cases the values of a and b are considerable fractions of e , that the great majority of the valuations hitherto made and published of the electromotive forces of various two-fluid cells cannot be properly compared with the heat-evolutions due to the net chemical actions taking place in the cells; because no accurate valuations having been made of the strengths of the solutions used, and the precise character of the plate-surfaces employed not having been noted, the values of the terms a and b cannot be calculated with precision so as to enable the values of E to be deduced for some given standard-strength of solution (*e. g.* for strength MSO_4 100 H_2O , or MCl_2 100 H_2O , &c.). Differences of several centivolts, and even decivolts, exist between the values assigned to certain voltaic combinations by different observers, mainly due to this cause. Accordingly it becomes necessary to repeat most of the work of previous experimenters with due regard to these points before comparisons can be satisfactorily instituted. A large number of cells have therefore been examined during the last four years, under strictly comparable conditions, in order to obtain sufficiently accurate values for the purpose of tracing out the correlations subsisting in different cases between the heat-evolution due to the net chemical change and the maximum E.M.F. actually developed, when so small a current flows as to cause no appreciable diminution by "polarization."

All these observations completely confirm the conclusions already arrived at, that *Volta's law of summation holds* within the limits of experimental error, and that *the effect of a given alteration in the molecular strength of a saline solution surrounding one of the plates of a two-fluid cell is sensibly independent of the nature of the other fluid and the other plate*. It hence becomes possible to assign numerical values or constants to given metals immersed in solutions of their salts of given nature and strength, such that the E.M.F. of a voltaic combination formed by combining two such immersed metals (by allowing the two solutions to interdiffuse, *e. g.* in a Raoult cell

* With cells set up with acid fluids (*e. g.* dilute sulphuric acid) surrounding the plate acquiring the lower potential, increasing the strength of the acid fluid not infrequently causes an increment, and not a decrement, in the E.M.F. of the combination; *e. g.* in Daniell cells (§§ 112-114, Part V.).

or in a "gravity" cell) is calculable by taking the difference between the constants; *i. e.* by employing the formula

$$E = C_1 - C_2,$$

where E is the E.M.F. of the combination, and C_1 and C_2 the two constants involved, such that if $C_1 - C_2$ is of + sign the metal to which C_1 applies acquires the higher potential, whilst if $C_1 - C_2$ is of - sign the metal to which C_2 applies acquires the higher potential. For instance, if C_2 apply to zinc and C_1 to copper in sulphate solution, $C_1 - C_2 = +1.114$, and copper acquires the higher potential; whilst if C_1 refer to magnesium, $C_1 - C_2 = -.725$, and zinc acquires the higher potential.

165. The value of the *voltaic constant* thus applicable to any given metal is variable, within certain limits, with its surface-conditions (according as bright fused metal, electro-deposited, amalgamated, &c.), and also with the nature and strength of the solution of its salt in which it is immersed, and probably also with the temperature, but is independent of the nature of the other half of the cell. In order to fix the numerical value some convention must be made as regards the zero to be adopted as starting-point. For this purpose, and in view of the comparisons desired to be instituted between the values of $C_1 - C_2$ and the heat-evolutions due to the net chemical actions taking place, the most convenient assumption appears to be this:—that the potential of an amalgamated zinc plate immersed in a solution of a given salt of zinc of given molecular strength is assumed to be zero, for the purpose of fixing the numerical value of the potential of a plate of the metal to be examined *when immersed in an equally strong solution of the corresponding salt of that metal*, the two solutions being allowed to interdiffuse (*e. g.* in a "gravity" cell or in a "Raoult" cell). For instance, when plates of electro-copper or cadmium are respectively immersed in solutions of the sulphates of these metals, and opposed to amalgamated zinc immersed in zinc-sulphate solution, the molecular strength throughout being $1.0 \text{ M SO}_4 \text{ } 100 \text{ H}_2\text{O}$, the potentials of the copper and cadmium plates are respectively $+1.114$ and $+.360$ volt higher than the potential of the zinc plate; which values are accordingly the "voltaic constants" for electro-copper in $1.0 \text{ Cu SO}_4 \text{ } 100 \text{ H}_2\text{O}$, and for electro-cadmium in $1.0 \text{ Cd SO}_4 \text{ } 100 \text{ H}_2\text{O}$ respectively: whence it results that the E.M.F. of a cell set up with electro-copper and electro-cadmium plates and solutions of the sulphates of these metals of molecular strength $1.0 \text{ M SO}_4 \text{ } 100 \text{ H}_2\text{O}$ will be

$$E = C_1 - C_2 = 1.114 - .360 = .754,$$

which in point of fact is the case. On the other hand, when magnesium is opposed in the same way to zinc in sulphate

solutions of strength 1.0 MSO_4 $100 \text{ H}_2\text{O}$, the former metal acquires a potential lower by $\cdot 725$ volt than that of the latter; whence it results that $-\cdot 725$ is the voltaic constant for magnesium in sulphate solution of this strength; wherefore the potential difference set up between magnesium and copper in sulphate solutions of this strength will be $1.114 - (-\cdot 725) = 1.839$ volt, which in practice is found to be the case. Evidently Volta's law of summation is involved in the proposition that the E.M.F. of a voltaic combination is the algebraic difference between the values of the voltaic constants as thus defined.

If the value of a given voltaic constant applying to a given metal immersed in a solution of one of its salts of given strength be known, that for any other given strength is readily calculable by means of the formula $E = e + a - b$ when tables of the values of a and b are extant, like those given in Part VIII. for certain kinds of plate-surfaces in chloride and sulphate solutions.

Out of a large number of voltaic combinations examined, consisting of two metals immersed in solutions of their salts of a given kind (*e. g.* sulphates, chlorides, nitrates, or acetates), only a few have been found to possess an E.M.F. which, when at its maximum, differs by but little from the value calculated from the heat-evolution during the net chemical action taking place in the cell, *i. e.* from the difference between the heats of formation of the two salt solutions forming the electrolytes; the great majority either fall short of this value, or exceed it by quantities well outside of the limits of experimental error. In cells of the latter class, it is evident that the extra work which can be done by the passage of a current through a large external resistance over and above that due to the net chemical change, must be accomplished by a transformation of sensible heat into electric energy; *i. e.* it is done at the expense of sensible heat, such cells resembling in this respect various of the diffusion-cells described in Part VIII. A still more remarkable class of cells has been found, however, in which the current flows in the direction *opposite* to that deducible from the relative heats of formation of the electrolytes, so that, instead of the plate immersed in the solution of the salt of greater heat of formation acquiring the lower potential (as the zinc plate in a Daniell cell), it actually acquires the higher potential. The passage of a current in such cases, consequently, is accompanied by a twofold transformation of sensible heat into current energy, actual or potential; for the chemical changes that take place in the cell are accompanied on the whole by heat-absorption, whilst any work done externally must be

performed at the expense of sensible heat*. Twenty such combinations have been examined, *vide* § 209.

166. An attentive consideration of these and other allied phenomena leads to the conclusions that, since only in exceptional cases does the formula hold,

$$E_H = E = C_1 - C_2$$

(where E_H is the E.M.F. corresponding to the net heat-evolution due to chemical change in the cell), *the E.M.F. actually generated must be due to other causes in addition to the heat of chemical change*; and that all the observed phenomena may be clearly accounted for if it be assumed that the difference of potential set up between the two plates of the voltaic combinations examined *is due to the superposition of two causes*—one, the net heat-development or difference in heat of formation of the two electrolytes surrounding the two plates respectively; the other, an action akin to that taking place in a thermoelectric combination, and, like that action, expressible numerically by the algebraic difference between two constants applicable to the two halves of the combination respectively; so that if k_1 and k_2 be the two *thermovoltaic* constants applicable respectively to the metals (and corresponding with the voltaic constants C_1 and C_2), and E_H be the E.M.F. corresponding with the difference between these heats of formation, then

$$E = C_1 - C_2 = E_H + k_1 - k_2.$$

If, then, $k_1 - k_2$ does not differ greatly from zero, E and E_H are nearly equal; if, on the other hand, $k_1 - k_2$ has a considerable value, E exceeds or falls short of E_H according as that value is positive or negative in sign. If $k_1 - k_2$ is negative in sign and also is numerically greater than E_H , E is of the opposite sign to E_H . This case corresponds with the class of cells above referred to in which the metal actually acquiring the higher potential is the one which would be expected to acquire the lower one, were the heat of chemical action the sole cause of potential difference in the cell.

The value of the thermovoltaic constant applicable to any given metal necessarily varies in the same way as that of the voltaic constant; *i. e.* it is to some extent dependent on the nature of the metal surface, and varies with the nature and strength of the solution of the salt of the metal employed to

* A somewhat analogous phenomenon is presented by gravity-diffusion cells set up with platinum plates and nitric acid of different degrees of concentration. With certain strengths an E.M.F. is set up superior to that calculable from the heat of intermixture of the two acid fluids; so that when a current flows through a large external resistance, part of the work done is performed at the expense of sensible heat; whilst the passage of the current reduces the concentrated acid to lower oxides of nitrogen (precisely as in a Grove's cell), thereby absorbing additional heat.

surround the plate, and probably also with the temperature*. In some few cases the constant is of positive sign for some solution-strengths or kinds of salts, and negative for others: in these cases the numerical value of the thermovoltaic constant is never great, not exceeding a few centivolts at most. If, on the other hand, a metal possess a thermovoltaic constant of considerable magnitude for one class of salts, the sign is the same for all classes of salts examined. So that practically metals may be divided into three classes, viz.:—those where the thermovoltaic constant has uniformly a more or less considerable negative value, *e. g.* lead and silver; those where the value is sometimes positive and sometimes negative but never large, *e. g.* copper and cadmium; and those where the value is considerable and always positive, *e. g.* iron, mercury, magnesium, and aluminium.

In order to have a standard for reduction, it is convenient to make the same convention as that above referred to for voltaic constants, viz. that the thermovoltaic constant for amalgamated zinc is taken as zero *when immersed in a solution of a zinc-salt corresponding in nature and molecular strength with the solution of the salt surrounding the other plate.*

As regards the actual numerical values assigned below to the voltaic and thermovoltaic constants, it has been thought best to adhere to the same fundamental assumptions as have been hitherto made throughout this series of papers, viz.:—that the B.A. unit of resistance is actually one earth-quadrant per second; that the factor for converting gramme-degrees into volts is 4410; and that the E.M.F. of Clark's cell at $15^{\circ}5$ is 1.457 volt, that of a Daniell cell set up with amalgamated-zinc and electro-copper plates and solutions of zinc and copper sulphates of equal molecular strength being $0.765 \times 1.457 \pm 0.0005$, or 1.114 ± 0.0005 volt. The present state of our knowledge would introduce corrections in all three of these values; but these corrections would so far balance one another that the most probable end result is that all the numerical values given are about 1.6 to 2.2 per cent. too high. First,

* S. Czapski has recently found (*Annalen der Physik*, xxi. p. 209) that zinc-silver-chloride and cadmium-silver-chloride cells diminish in E.M.F. as the temperature rises; whilst iron-mercurous-chloride, cadmium-mercurous-chloride, and zinc-mercurous-bromide cells rise in E.M.F. with increasing temperature. In the former class of cell the E.M.F. actually set up is less, and in the latter greater, than that corresponding with the heat-evolution due to the net chemical change. But it would seem that if there be any general law connecting the variations in E.M.F. with temperature, it is less simple than might appear from these particular results; for Clark's cells (zinc-mercurous-sulphate) obviously belong to the same class as zinc-mercurous-bromide and cadmium-mercurous-chloride cells; and yet the E.M.F. of a Clark's cell *falls* instead of rising as the temperature rises (§§ 139 and 179).

the true value of the B.A. unit is about 1·3 per cent. lower than that thus assumed; next, Lord Rayleigh's recent valuations of the E.M.F. of Clark's cell indicate 1·434 instead of 1·457, or 1·6 per cent. lower (this correction including the previous one). Still smaller values, 1·427 to 1·433 volt, or from 1·7 to 2·1 per cent. lower than Clark's value, have recently been obtained by Von Ettingshausen (*Zeitsch. für Electrotechnik*, 1884, Heft xvi.); whilst, lastly, the value of JF , the factor for converting heat units into volts, as deduced from the most recent experiments of Mascart, Kohlrausch, and Lord Rayleigh, is nearly as much below 4410; these three observers finding for the C.G.S. electrochemical equivalent of silver respectively the values ·01124, ·011183, and between ·01118 and ·01119, from which a mean value of close to ·0112

results; so that $F = \frac{·0112}{107·93} = ·0001038$; whence, taking

$J = 41·55 \times 10^6$, it follows that $JF = 4313$, or 2·2 per cent. below 4410. So that, on the whole, both E and E_H are uniformly overvalued below by amounts probably amounting to between 1·6 and 2·2 per cent.; whence their differences will evidently be overvalued to sensibly the same extent. The errors thus introduced into the values assigned to the various thermovoltaic constants calculated are in no case of such magnitude as to interfere with the general character of the inferences drawn from the numbers thus deduced.

167. A number of cells have been examined, in which a sparingly soluble electrolyte was employed in the form of the solid salt suspended in a solution of some other salt (usually the one surrounding the other plate), *e.g.* zinc-silver-chloride cells, where the silver plate was surrounded by a magma of silver chloride suspended in zinc-chloride solution; or cadmium-lead-sulphate cells, where lead sulphate suspended in cadmium-sulphate solution surrounded the lead plate. Some noteworthy peculiarities have been observed with such cells as regards the fluctuations in the values of a and b in the formula $E = e + a - b$ with variations in solution-strength. Thus, with zinc-lead-sulphate cells (set up with a magma of lead-sulphate and zinc-sulphate solution round the lead plate) the effect of variation in the strength of the zinc-sulphate solution surrounding the lead plate is not the same, either in magnitude or sign, as the effect of the same variation in the strength of the zinc-sulphate solution surrounding the zinc plate. In the latter case (as already shown, Part VIII.) the effect is of this kind, that as the solution-strength increases the numerical value of b continually increases, its sign being always the same, *viz.* positive; but in the former case this is not so, the numerical value of a increasing with the strength

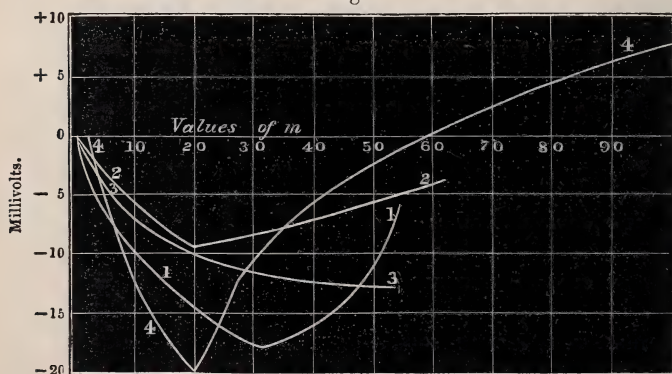
of the zinc-sulphate solution in which the lead sulphate is suspended up to a certain strength, after which the value again decreases, the sign being *negative* throughout. Thus the following Table represents the relative values of a and b for solution-strengths $m\text{ZnSO}_4 \cdot 100 \text{H}_2\text{O}$, the values of b being those described in § 153 for amalgamated-zinc plates, and those of a similarly obtained by means of a series of diffusion-cells set up with electro-coated lead plates immersed in magmas of recently precipitated well-washed lead-sulphate and zinc-sulphate solutions of varying strengths; the values are given in millivolts.

Value of m .	a .	b .
·1	0	0
·5	— 6·0	+11·1
1·0	— 9·5	+16·3
2·0	—14·0	+22·6
3·0	—18·0	+27·7
4·0	—16·5	+33·9
5·0	—11·0	+39·5
5·5	— 5·5	+43·5

Curve no. 1, fig. 1, indicates these values of a as ordinates, the values of m being abscissæ.

On substituting solutions of cadmium sulphate for those of zinc sulphate as the menstrua in which to suspend lead sulphate in cells containing lead and its sulphate as one half of the element, analogous results were obtained as regards the general character of the curve, the mean numerical values of a obtained by means of diffusion-cells being given in millivolts in the following Table, and represented by curve no. 2;

Fig. 1.



the third column in the table and curve no. 3 represent the corresponding values obtained with diffusion-cells containing

mercurous sulphate suspended in zinc-sulphate solutions of varying strengths, the plate-surfaces being in this case pure mercury (the "plates" being the upper surfaces of mercury occupying the lowest portions of the vessels constituting the cells).

Values of m .	Values of a in cells containing lead sulphate suspended in $m\text{CdSO}_4$ 100 H_2O .	Values of a in cells containing mercurous sulphate suspended in $m\text{ZnSO}_4$ 100 H_2O .
·1	0	0
·5	-2·3	- 5·0
1·0	-5·2	- 6·6
2·0	-9·2	- 9·5
3·0	-8·1	-11·6
4·0	-6·9	-12·2
5·0	-5·6	-12·7
5·5	-5·0	-13·0
6·0	-4·3	

It is here evident that whilst the sign of a is in all cases negative, the numerical value attains a maximum in the case of lead sulphate suspended in either zinc- or cadmium-sulphate solution and then diminishes, whereas it continually increases in the case of mercurous sulphate suspended in zinc sulphate. On the other hand, cells containing electro-silver plates immersed in magmas of recently precipitated well-washed silver chloride and solutions of zinc chloride of varying strengths, gave values for a negative in sign for low solution-strengths, and increasing in numerical value to a negative maximum as the strength increased, after which the value diminished again to 0, finally becoming positive, and increasing continuously; these values are given in millivolts in the following table, and indicated by curve no. 4.

Value of m .	Value of a in cells containing silver chloride suspended in $m\text{ZnCl}_2$ 100 H_2O .
·25	0
·5	- 5·0
1·0	-12·0
2·0	-20·1
3·0	-10·2
6·0	+ 0·1
10·0	+ 7·0

Analogous values were obtained with cells containing mercurous chloride suspended in zinc-chloride solution, a being negative in sign with strengths where $m < 6·0$, but positive where $m = 10·0$.

168. It is evident that cells of the type now under discussion, where the electrolyte surrounding the plate acquiring

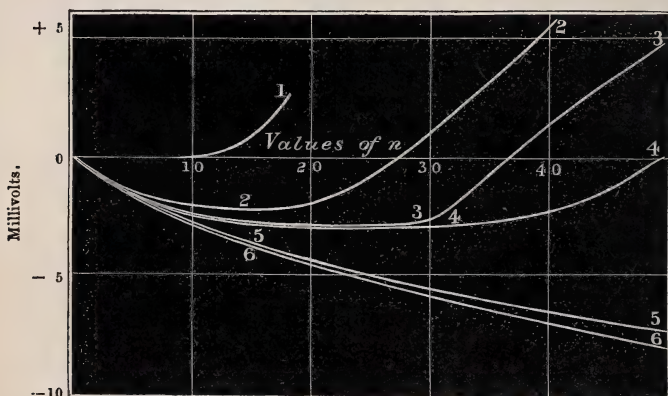
the higher potential is of a mixed character (*i. e.* containing two salts), are to a certain extent comparable with Daniell cells in which the copper plate is surrounded by a mixed solution of zinc and copper sulphates, the only essential difference being in the greater solubility of copper sulphate as compared with lead sulphate, &c. It seemed of interest, therefore, to examine in detail the influence on the numerical value of the E.M.F. of a Daniell cell exerted by addition of zinc sulphate to the copper-sulphate solution used. Accordingly a number of cells were set up containing mixed sulphate solutions of strengths $m\text{CuSO}_4, n\text{ZnSO}_4, 100\text{H}_2\text{O}$, the values of m and n varying in the different cases, but all else being the same. The following remarkable results were thus obtained, electro-copper plates being used throughout:—

1. With solutions such that m constantly = 1.77, the value of a with continually increasing values of n is never negative, being sensibly = 0 as long as n does not exceed .5, and being +, and continually increasing in numerical value as n increases above .5.

2. With solutions such that m does not exceed .5, the value of a is always negative, increasing in numerical value as n increases.

3. With solutions such that m = from 1.0 to 1.42, the value of a is at first negative, increasing to a negative maximum as n increases, then diminishing to zero, and finally becoming positive, and continuously increasing in numerical value.

Fig. 2.



The following tables represent these results, which are also indicated by curves 1 to 6 (fig. 2); the values are given in millivolts.

Value of $m=1.77$. (Curve No. 1.)		
Value of n .	E.M.F. of cell.	Value of a .
0	1114.0	
.1	1114.0	0
.5	1114.0	0
1.0	1114.1	+ .1
1.5	1114.7	+ .7
2.0	1117.9	+3.9
$m=1.42$. (Curve No. 2.)		
0	1114.0	
.5	1112.6	-1.4
1.0	1112.0	-2.0
1.5	1111.9	-2.1
2.0	1112.1	-1.9
3.0	1115.0	+1.0
4.0	1119.5	+5.5
$m=1.28$. (Curve No. 3.)		
0	1114.0	
1.0	1111.8	-2.2
1.5	1111.0	-3.0
3.0	1110.9	-3.1
4.0	1115.2	+1.2
5.0	1118.7	+4.7
$m=1.00$. (Curve No. 4.)		
0	1114.0	
.25	1113.3	- .7
.5	1112.6	-1.4
1.0	1111.7	-2.3
1.5	1111.0	-3.0
2.0	1110.9	-3.1
3.0	1110.8	-3.2
4.0	1111.9	-3.1
5.0	1114.2	+ .2
$m=.5$. (Curve No. 5.)		
0	1114.0	
.1	1113.6	- .4
.25	1113.3	- .7
.5	1112.6	-1.4
1.0	1111.5	-2.5
2.0	1110.0	-4.0
4.0	1107.7	-6.3
5.0	1106.7	-7.3
$m=.25$. (Curve No. 6.)		
0	1114.0	
.25	1113.1	-0.9
1.0	1111.4	-2.6
3.0	1108.4	-5.6
5.0	1106.1	-7.9

Hence it appears that cells containing very sparingly soluble compounds like lead sulphate, silver chloride, &c., suspended in solutions of more soluble salts, are comparable with Daniell cells in which the copper plates are surrounded with solutions containing only small quantities of copper sulphate, together with varying quantities of zinc sulphate, increasing the strength of the solution of the more soluble salt, in the first case producing the same general effect on the E.M.F. of the cell as addition of more zinc sulphate to the copper-sulphate solution in the second.

169. A probable explanation of these results is the following:—The passage of a current, however feeble, decomposes both electrolytes in the mixed fluid simultaneously; so that, for example, in the Daniell cell containing mixed zinc- and copper-sulphate solution round the copper plate, both zinc and copper would be thrown down simultaneously in the metallic state, were it not that whilst still nascent the zinc acts on the copper-sulphate solution by a secondary action, precipitating the equivalent amount of copper, and becoming itself zinc-sulphate solution. A portion of the energy gained by this secondary action is nonadjuvant, the amount of nonadjuvant energy depending, *inter alia*, on the number of molecules of zinc sulphate thus reproduced relatively to that of copper sulphate in solution, in such a fashion that *the stronger the copper-sulphate solution, cæteris paribus, the less is the amount of nonadjuvant energy*. Hence, from this cause the E.M.F. of a Daniell cell would be necessarily diminished by addition of zinc sulphate to the copper-sulphate solution. But this addition produces also an effect of another kind; for addition of zinc sulphate to the fluid surrounding the plate acquiring the higher potential, making that fluid stronger so far as zinc sulphate is concerned, must set up an interdiffusion effect which will be superposed upon the other sources of difference of potential, tending to raise the potential of the plate immersed in the fluid thus strengthened relatively to that of the other plate. Hence there are two causes, acting in opposite directions, thus brought into play, tending to affect the potential-difference set up between the two plates of the cell: if the one predominate, the E.M.F. of the cell as a whole is lowered; if the other predominate, the E.M.F. is raised. With weak solutions of copper sulphate the former tendency is always greater than the latter, and the E.M.F. falls continuously as zinc sulphate is added to the copper-sulphate solution, as indicated in curves nos. 5 and 6, fig. 2. With strong solutions of copper sulphate the latter tendency is

always greater than the former, so that in this case the E.M.F. continuously increases in value as zinc sulphate is added to the copper sulphate, as indicated in curve no. 1. With intermediate strengths of copper-sulphate solution, the former tendency at first predominates over the latter one, the E.M.F. of the cell falling as more zinc sulphate is intermixed with the copper sulphate; but by and by the second tendency becomes stronger relatively to the first, and finally overpowers it, thus causing the E.M.F. of the cell first to cease diminishing, and then to rise continuously until it reaches and even surpasses the first value.

Mutatis mutandis, evidently the same kind of explanation applies in the case of cells where the mixed electrolyte is a magma of a sparingly soluble salt suspended in a solution of a more soluble one.

It should hence evidently result that the effect of "polarization" on cells set up with such magmas (*i. e.* the depreciation in E.M.F. observed with increasing current-density) should be much more marked than with cells containing only readily soluble salts in moderately concentrated solution; since the film of fluid surrounding the plate on which metal is deposited must, with such a magma, become very rapidly depreciated, as regards the amount in solution of the sparingly soluble salt, on account of the decomposition of that salt caused by the passage of the current. This is, in fact, the case, depreciation of some tenths of a millivolt being readily observable even with currents of density too small to produce any measurable effect on such diffusion-cells as those described in Part VIII. In order to avoid errors from this cause exceeding 0.5 millivolt in magnitude, it was found indispensable to work with currents of density not exceeding from .01 to .02 microampere per square centimetre when cells containing such magmas were employed; so that with plates exposing some 3 or 4 square centimetres of surface, and cells of near to 1 volt in E.M.F., a total resistance of at least a megohm, and usually of several megohms, was employed; and proportionately in other cases.

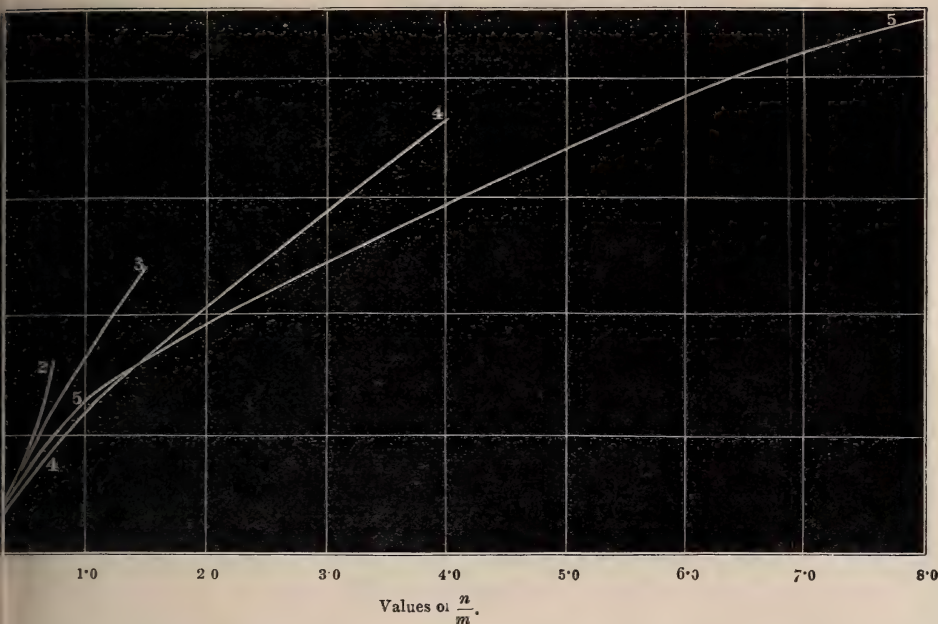
It should further result, if the above explanation be correct, that if two cells be compared, alike in all respects save that in one the copper plate is surrounded by a mixed solution $m \text{ CuSO}_4$, $n \text{ ZnSO}_4$ 100 H_2O , and in the other by a solution of copper sulphate only, of equal strength, viz. $(m + n) \text{ CuSO}_4$ 100 H_2O , the difference between the E.M.F.'s of the two for a given value of the ratio $\frac{n}{m}$ will not be constant, but will

increase as the value of m diminishes, *i. e.* as the mixed solution is weaker so far as the copper sulphate contained is concerned. This is in fact the case, as shown by the following Table, representing the E.M.F.'s of such pairs of cells uniformly containing amalgamated-zinc and electro-copper plates, the former immersed in $m \text{ ZnSO}_4$ 100 H_2O ; the E.M.F.'s of the cells containing $m \text{ CuSO}_4$, $n \text{ ZnSO}_4$ 100 H_2O are derived from the preceding tables, and those of the cells containing $(m+n) \text{ CuSO}_4$ 100 H_2O are calculated by means of the formula $E=1114+a$, in which 1114 is the constant E.M.F. in millivolts of an ordinary Daniell cell containing $m \text{ MSO}_4$ 100 H_2O , where m is constant for both zinc- and copper-sulphate solutions (§ 170, footnote), whilst a is derived from the table given in Part VIII. § 154.

m .	n .	E.M.F. of cells containing		Differ- ence.	$\frac{n}{m}$.
		$(m+n) \text{ CuSO}_4$ 100 H_2O .	$m \text{ CuSO}_4$, $n \text{ ZnSO}_4$ 100 H_2O .		
1.77	.5	1116.9	1114.0	2.9	.28
1.42	.5	1116.9	1112.6	4.3	.35
1.42	1.0	1120.2	1112.0	8.2	.70
1.00	.25	1115.6	1113.3	2.3	.25
1.00	.5	1117.2	1112.6	4.6	.50
1.00	1.0	1120.4	1111.7	8.7	1.0
1.00	1.5	1123.4	1111.0	12.4	1.5
.5	.1	1114.9	1113.6	1.3	.2
.5	.25	1116.3	1113.3	3.0	.5
.5	.5	1118.6	1112.6	6.0	1.0
.5	1.0	1121.8	1111.5	10.3	2.0
.5	1.5	1125.0	1110.7	14.3	3.0
.5	2.0	1128.0	1110.0	18.0	4.0
.25	.25	1119.6	1113.1	6.5	1.0
.25	1.0	1125.8	1111.4	14.4	4.0
.25	2.0	1132.1	1109.9	22.2	8.0

Fig. 3 represents these numbers, the values of $\frac{m}{n}$ being the abscissæ and those of the "differences" the ordinates of the curves drawn, which are evidently such that the curve where m has the highest value overlies the others; and similarly throughout.

Fig. 3.



Determination of the Numerical Values of the Voltaic Constants and Thermovoltaic Constants applicable to various kinds of Two-fluid Cells.

170. In all the cells examined, wherever possible, the plates used were coated over by an electro-deposited coat of metal, thrown down by a moderately weak current from a strong solution of a pure salt of the metal. The plates employed usually exposed a total surface of 3 or 4 square centimetres, and the electro-coating was effected by passing the current from three to five Leclanché quart-cells in series through a decomposing cell, in which the plate to be coated formed the negative electrode, the positive one being a plate of the same metal in as great a state of purity as practicable; the electrodes were usually some 3 or 4 centimetres apart. Under these circumstances the coating of metal formed was fairly tough and coherent, although in certain cases somewhat crystalline on the surface. Copper thus deposited from a strong solution of copper sulphate formed a coating of great regularity and toughness. In some cases, however, bright plates of fused metal were necessarily employed, *e. g.* with iron and aluminium. With zinc the plates always consisted

of pure metal superficially amalgamated with pure mercury. The cells employed were uniformly constructed of two small beakers or porcelain vessels containing respectively the metals and fluids employed, and united by a siphon-tube, consisting of an inverted Y-tube, the lower ends of which were covered over with thin animal membrane secured by a silk thread, and rendered watertight, when requisite, at the junction of the glass and membrane by the application of a little melted gutta-percha, so that no leakage into or from the siphon took place, saving by osmosis through the membrane. In all cases the temperature varied from 18° C. by only \pm a few degrees. Saving in the case of certain experiments quoted from previous portions of these researches, and a few others made for special reasons with the quadrant-electrometer, all the observations were made by means of the galvanometer, currents being used of such small density as to produce no sensible depreciation in the E.M.F.; in most cases an external resistance of a megohm (and in many of much more) being employed, the valuation being obtained by comparing the readings of the cell to be examined with those of an Alder-Wright's normal Daniell cell* through the same resistance, using the formula

$$E = 1.114 \times \frac{m}{n},$$

where m is the average reading of the cell to be valued, and n that of the standard Daniell. Usually alternate readings of the two cells were taken for 20–30 minutes after first setting up, and averaged to obtain the values of m and n .

A. Voltaic and Thermovoltaic Constants of Metals immersed in Solutions of their Sulphates.

I. Copper.

171. Electro-copper presents the remarkable and almost

* The term "normal Daniell cell" has been applied by different observers to very different forms of cell, varying several per cents. in E.M.F. amongst themselves, principally according as sulphuric acid of one strength or another, or zinc-sulphate solution saturated or otherwise, is used to surround the zinc plate. What is here referred to as an "Alder-Wright's normal Daniell" is a cell set up after Raoult's form of construction, with amalgamated-pure-zinc and electro-copper plates immersed in pure solutions of their respective sulphates of the same molecular strength, preferably of strength m MSO_4 100 H_2O , where m is near to 2.0, i. e. with copper-sulphate solution nearly but not absolutely saturated, so as not to deposit crystals by chilling or slight evaporation. The ratio of the E.M.F. of such a cell to that of a Clark's cell at $15^{\circ}.5$ has been found to be sensibly .765 to unity.

unique peculiarity, that the value of its voltaic constant in sulphate solution, as above defined, is practically uniform for all solution-strengths; for, as already shown, the E.M.F. of a zinc-copper-sulphate cell is sensibly the same whatever the solution-strength when amalgamated-zinc and electro-copper plates are used, viz. (on the assumptions above discussed, § 166) $1.114 \pm .0005$ volt; so that the voltaic constant for electro-copper in sulphate solution is sensibly $+1.114$ for all solution-strengths (*i. e.* when referred to amalgamated zinc in zinc-sulphate solution of the same strength as zero).

The thermovoltaic constant of electro-copper also shows the same independence; for the experiments described in § 160 show that the heats of dilution of zinc- and copper-sulphate solutions are sensibly identical for equal dilution-ranges; hence the heats of formation of zinc- and copper-sulphate solutions both of strength m $\text{MSO}_4 \cdot 100\text{H}_2\text{O}$ (where m is the same) must differ by a constant amount whatever the value of m : this amount for $m = .25$ (and therefore for all other values of m) is, from Thomsen's results:—

$$\begin{array}{r} \text{Zn, O, SO}_3 \text{ aq.} = 106090 \\ \text{Cu, O, SO}_3 \text{ aq.} = 55960 \\ \hline 50130 = 1.105 \text{ volt.} \end{array}$$

Hence, finally, since $E = 1.114$ and $E_H = 1.105$ for all solution-strengths, the thermovoltaic constant for electro-copper in sulphate solution is constantly $1.114 - 1.105 = +.009$ for all solution-strengths.

II. Cadmium.

172. The experiments with the quadrant-electrometer described in Part VI. show that the electromotive forces of all zinc-cadmium-sulphate cells set up with solutions of zinc and cadmium sulphate of the same strength, m $\text{MSO}_4 \cdot 100\text{H}_2\text{O}$, are comprised within the limits $.360 \pm .0015$, when m is between .08 and 2.0, and that those of the analogous copper-cadmium-sulphate cells are similarly comprised within the limits $.7525 \pm .0015$.

The experiments described in Part VIII., however, enable still more exact figures to be deduced. Applying the formula $E = e + a - b$, and taking the values of a and b from the tables quoted, the following figures result as the most probable ones, the probable error in no case being so great as $\pm .002$ volt:—

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Value of m .	E.M.F. of zinc-cadmium cell.	E.M.F. of cadmium-copper cell.	Voltaic constant for electro-cadmium.
·1	·362	·752	+·362
·25	·362	·752	+·362
·5	·361	·753	+·361
1·0	·360	·754	+·360
2·0	·359	·755	+·359
3·0	·358		+·358
4·0	·3575		+·3575
5·0	·357		+·357

Unlike that of copper therefore, the voltaic constant for electro-cadmium immersed in m CdSO_4 $100\text{H}_2\text{O}$, referred to amalgamated zinc in zinc-sulphate solution of the same strength as zero, *varies with the value of m , decreasing as m increases.*

From these values the corresponding thermovoltaiic constants are thus calculable. According to Julius Thomsen the heats of formation of zinc and cadmium sulphates in solution of strength ·25 MSO_4 $100\text{H}_2\text{O}$ differ by 16210 :—

$$\begin{array}{r} \text{Zn, O, SO}_3 \text{ aq.} = 106090 \\ \text{Cd, O, SO}_3 \text{ aq.} = 89880^* \\ \hline 16210 \end{array}$$

The heats of dilution to this strength of more-concentrated solutions are not equal in the two cases : calling them respectively h_1 and h_2 , the heat of displacement per gramme-molecule of cadmium from m CdSO_4 $100\text{H}_2\text{O}$ by zinc is calculable by the formula

$$H = 16210 - (h_1 - h_2),$$

when m , h_1 , and h_2 have the following values :—

m .	h_1 .	h_2 .	$16210 - (h_1 - h_2)$.	Corresponding E.M.F.
·25	0	0	16210	·357
·5	0	25	16235	·358
1·0	0	75	16285	·359
2·0	50	275	16435	·362
3·0	100	450	16560	·365
4·0	175	625	16660	·367
5·0	375	825	16660	·367

Hence the values of the thermovoltaiic constants for electro-

* *Thermochemische Untersuchungen*, vol. iii. p. 285. In the calculations given in Part VI. the value 89500 is assigned, taken from Thomsen's earlier publications (*Journ. prakt. Chem.* xi. p. 401).

cadmium immersed in $m\text{CdSO}_4 \cdot 100\text{H}_2\text{O}$ (referred to amalgamated zinc immersed in equally strong zinc-sulphate solutions) are positive for weak solution-strengths and negative for considerably strong solutions, being in no case numerically great, viz. as follows:—

m .	E.	E_H .	Thermovoltaiic constant.
·25	·362	·357	+·005
·5	·361	·358	+·003
1·0	·360	·359	+·001
2·0	·359	·362	—·003
3·0	·358	·365	—·007
4·0	·3575	·367	—·0095
5·0	·357	·367	—·010

III. Silver.

173. The experiments already described in Part VI. show that the E.M.F.'s of zinc-silver-sulphate cells and the corresponding cadmium- and copper-silver cells are respectively 1·536, 1·1805, and ·4235, when amalgamated-zinc and electro-silver, -copper, and -cadmium plates are used with solutions of ·042 $\text{MSO}_4 \cdot 100 \text{H}_2\text{O}$ throughout. The probable error attaching to these values was in each case between $\pm \cdot 001$ and $\pm \cdot 0015$. Hence, referred to amalgamated zinc in zinc-sulphate solution of this strength as zero, the following values result for the voltaic constant of electro-silver in saturated silver-sulphate solution:—

From Zinc-Silver cells	1·536
From Cadmium-Silver + Zinc-Cadmium cells	$1·1805 + \cdot 362 = 1·5425$
From Copper-Silver + Zinc-Copper cells..	$1·114 + \cdot 4235 = 1·5375$
Mean	1·539

These three valuations do not accord so well as those obtained in most of the experiments subsequently detailed and made by means of the galvanometer; the chief causes of the discrepancies lie in the greater want of permanence and wider range of fluctuation noticed with these cells (*vide* §§ 125–128, Part VI.), probably due to the extremely weak solutions necessarily employed owing to the sparing degree of solubility of silver sulphate.

Taking the mean value 1·539 volt as the voltaic constant for electro-silver in saturated silver-sulphate solution, it results that the corresponding thermovoltaiic constant has a large negative value; for Julius Thomsen finds that Ag_2O ,

20 Messrs. Wright and Thompson on the Determination of SO_3 aq.=20390, whence the heat of displacement of silver from silver-sulphate solution by zinc is 85700 gramme-degrees=1.890 volt, so that the thermovoltaic constant

$$k = E - E_H = 1.539 - 1.890 = -.351.$$

IV. Lead.

174. A number of cells were examined set up with well-washed recently precipitated lead sulphate (prepared from clear lead-acetate solution and pure sulphuric acid in excess, boiled up many times with distilled water, and washed by decantation), suspended in zinc- or cadmium-sulphate solutions of varying strength. Electro-coated lead plates immersed in these magmas were opposed to amalgamated-zinc plates immersed in zinc-sulphate solution with the following results, to which are also joined the figures obtained with similar cells, in which electro-copper and electro-cadmium plates immersed in solutions of their sulphates respectively were opposed to the lead plates. The probable error of each value is in almost every instance considerably below $\pm .001$ volt, and in no case exceeds $\pm .002$ volt. The values refer to cells containing on both sides solutions of the same molecular strength $m \text{MSO}_4$ 100 H_2O .

<i>m.</i>	Lead sulphate suspended in zinc-sulphate solution.			Lead sulphate suspended in cadmium-sulphate solution.	
	Zinc-Lead.	Cadmium-Lead.	Lead-Copper.	Zinc-Lead.	Lead-Copper.
0.1	.537	.173	.577	.550	.564
0.5	.520	.160	.594	.537	.577
1.0	.512	.152	.603	.529	.585
2.0	.501	.142	.611	.519	.596
3.0	.491	.133		.514	
4.0	.487			.509	
5.0	.487			.505	
5.75	.487			.501	

From these various cell-values, together with those above described for zinc-copper and zinc-cadmium, the following valuations are deducible for the voltaic constants of electro-lead immersed in a magma of lead sulphate in $m \text{ZnSO}_4$ 100 H_2O , or in a magma of lead sulphate in $m \text{CdSO}_4$ 100 H_2O .

Lead Sulphate suspended in Zinc-sulphate Solution.

<i>m.</i>	Zinc-Lead.	Zinc-Cadmium + Cadmium-Lead.	Zinc-Copper - Copper-Lead.	Mean.
0.1	.537	.173 } + .362 } = .535	1.114 } - .577 } = .537	.536
.5	.520	.160 } + .361 } = .521	1.114 } - .594 } = .520	.520
1.0	.512	.152 } + .360 } = .512	1.114 } - .603 } = .511	.512
2.0	.501	.142 } + .359 } = .501	1.114 } - .611 } = .503	.502
3.0	.491	.133 } + .358 } = .491		.491
4.0	.487			.487
5.0	.487			.487
5.75	.487			.487

Lead Sulphate suspended in Cadmium-sulphate Solution.

<i>m.</i>	Zinc-Lead.	Zinc-Copper - Copper-Lead.	Mean.
0.1	.550	1.114 } - .564 } = .550	.550
0.5	.537	1.114 } - .577 } = .537	.537
1.0	.529	1.114 } - .585 } = .529	.529
2.0	.519	1.114 } - .596 } = .518	.5185
3.0	.514		.514
4.0	.509		.509
5.0	.505		.505
5.75	.501		.501

From these mean values for the voltaic constants the following numbers are deduced as the thermovoltic constants for electro-lead in contact with solid lead sulphate suspended in zinc- or cadmium-sulphate solution; the heat of displacement of lead from (solid) lead sulphate by zinc forming solution of zinc sulphate of strength $m \text{ ZnSO}_4 \text{ 100 H}_2\text{O}$ being

$$106090 - 73800 - h = 32290 - h;$$

where $106090 = \text{Zn, O, SO}_3 \text{ aq. } (m = .25),$

$73800 = \text{Pb, O, SO}_3 \text{ aq. (solid),}$

$h = \text{heat of dilution of } m \text{ ZnSO}_4 \text{ 100 H}_2\text{O to}$
 $.25 \text{ ZnSO}_4 \text{ 100 H}_2\text{O } (\S \text{ 160}).$

<i>m.</i>	<i>h.</i>	E_H	Lead sulphate suspended in zinc sulphate.		Lead sulphate suspended in cadmium sulphate.	
			E	$E - E_H$	E	$E - E_H$
0.1	0	·712	·536	—·176	·550	—·162
0.5	0	·712	·520	—·192	·537	—·175
1.0	0	·712	·512	—·200	·529	—·183
2.0	50	·711	·502	—·209	·5185	—·1925
3.0	100	·710	·491	—·219	·514	—·196
4.0	175	·708	·487	—·221	·509	—·199
5.0	375	·704	·487	—·217	·505	—·199
5.75			·487		·501	

Hence electro-lead in connexion with its sulphate is analogous to silver, the thermovoltaic constant possessing a considerable *negative* value; so that zinc-lead-sulphate and cadmium-lead-sulphate cells have E.M.F.'s notably below the values corresponding with the net chemical actions taking place therein. On the other hand, since the thermovoltaic constant for copper is a small positive value (+·009 for all solution-strengths), and that for lead is in all cases a considerably larger negative value, whilst copper is the metal acquiring the higher potential in a lead-copper-sulphate cell, it follows that the value of $k_1 - k_2$ in such a cell is always a considerable + quantity, varying from +·009—(—·162) to +·009—(—·221); that is from +·171 to +·230, according as the lead sulphate is suspended in zinc- or cadmium-sulphate solution, and according as the molecular strength of the solutions is low or high. In other words, a lead-copper-sulphate cell gives an effective maximum E.M.F. of from ·171 to ·230 volt *above that corresponding with the net chemical action taking place therein*. This chemical action represents a heat-evolution of $73800 - 55960 = 17840$ gramme-degrees per gramme-molecule, corresponding with ·393 volt only. The actually observed values consequently correspond to an increment of about 50 per cent. in the E.M.F. of the cell above that due to chemical action.

On the other hand, since the thermovoltaic constant for silver in contact with saturated silver-sulphate solution is a negative quantity numerically greater than that for lead, whilst in lead-silver-sulphate cells silver is the metal acquiring the higher potential, the value of $k_1 - k_2$ in such cells must be negative. Thus a cell set up with electro-silver immersed in saturated silver-sulphate solution opposed to electro-lead immersed in a magma of lead sulphate suspended in ·042 ZnSO_4 100 H_2O , must yield a value for $k_1 - k_2$ close to $-\cdot351 - (-\cdot176) = -\cdot175$; that is, the E.M.F. of such a

cell must be close to $1.1775 - .175 = 1.0025$ volt, since the heat of displacement of silver from silver-sulphate solution by lead forming solid lead sulphate is $73800 - 20390 = 53410$ gramme-degrees per gramme-molecule, equivalent to 1.1775 volt. On trying the experiment, the following values were observed with a number of similar cells :—

Maximum cell-value	=	1.008	volt.
Minimum	„	=	1.001 „
Average	„	=	1.003 „
Probable error	±	.0008	„

the average observed value being thus sensibly identical with the calculated one.

V. Iron.

175. A number of cells were examined set up with electro-copper plates immersed in copper-sulphate solution, opposed to plates of brightened sheet iron of as high a degree of purity as could be obtained, immersed in freshly prepared ferrous-sulphate solution ; and, similarly, numerous observations were made with analogous cells containing electro-cadmium plates immersed in cadmium-sulphate solution, amalgamated-zinc plates immersed in zinc-sulphate solution, or electro-lead plates immersed in a magma of lead sulphate suspended in zinc-sulphate solution. The following average values were obtained : with solutions of molecular strength throughout $.1 \text{ MSO}_4 100 \text{ H}_2\text{O}$, the cells exhibited far greater want of permanence and much wider fluctuations in E.M.F. than were noticed with cells of strength $1.0 \text{ MSO}_4 100 \text{ H}_2\text{O}$; so that, whilst the probable error in the former class of cells ranged from $\pm .003$ to $\pm .005$, that in the latter class was only from $\pm .001$ to $\pm .002$. The negative sign attached to the values for the iron-cadmium cells indicates that iron was the metal acquiring the higher potential, contrary to what might *à priori* be anticipated from the heats of formation, viz. $\text{Fe, O, SO}_3 \text{ aq.} = 93200$ and $\text{Cd, O, SO}_3 \text{ aq.} = 89880$ *.

Pairs of metals.	Molecular strength $1.0 \text{ MSO}_4 100 \text{ H}_2\text{O}$.	Molecular strength $0.1 \text{ MSO}_4 100 \text{ H}_2\text{O}$.
Zinc-Iron399	.445
Iron-Cadmium ..	— .035	— .073
Iron-Lead112	.120
Iron-Copper715	.685

* This peculiarity of iron-cadmium-sulphate cells has been already noticed by Braun (*Annalen der Physik*, xvi. p. 561).

These values, together with those above cited for zinc-cadmium, zinc-lead, and zinc-copper cells, give the following valuations for the voltaic constant for polished metallic iron in ferrous-sulphate solution :—

<i>m.</i>	Zinc-Iron.	Zinc-Cadmium— Cadmium-Iron.	Zinc-Lead— Lead-Iron.	Zinc-Copper— Copper-Iron.	Mean.
1.0	·399	$\left. \begin{array}{l} \cdot 360 \\ + \cdot 035 \end{array} \right\} \cdot 395$	$\left. \begin{array}{l} \cdot 512 \\ - \cdot 112 \end{array} \right\} \cdot 400$	$\left. \begin{array}{l} 1 \cdot 114 \\ - \cdot 715 \end{array} \right\} \cdot 399$	·398
0.1	·445	$\left. \begin{array}{l} \cdot 362 \\ + \cdot 073 \end{array} \right\} \cdot 435$	$\left. \begin{array}{l} \cdot 537 \\ - \cdot 120 \end{array} \right\} \cdot 417$	$\left. \begin{array}{l} 1 \cdot 114 \\ - \cdot 685 \end{array} \right\} \cdot 429$	·432

It results from these figures that the thermovoltaic constant for polished iron in ferrous-sulphate solutions must always have a notable positive value. Admitting, what is doubtless the case, that the heats of dilution of ferrous- and zinc-sulphate solutions from the molecular strengths $1 \cdot 0 \text{ MSO}_4 \cdot 100 \text{ H}_2\text{O}$ to $0 \cdot 25 \text{ MSO}_4 \cdot 100 \text{ H}_2\text{O}$ are not widely different, so that their difference is negligible, the heat of displacement of iron from $1 \cdot 0 \text{ FeSO}_4 \cdot 100 \text{ H}_2\text{O}$ by zinc is $106090 - 93200 = 12890$ gramme-degrees per gramme-molecule, corresponding with $\cdot 284$ volt. Since iron acquires the higher potential in zinc-iron-sulphate cells, the value of $k_1 - k_2$ must be positive and $= \cdot 398 - \cdot 284 = + \cdot 114$; whence the thermovoltaic constant for iron must be $+ \cdot 114$ for $1 \cdot 0 \text{ FeSO}_4 \cdot 100 \text{ H}_2\text{O}$; and must consequently have higher values still for weaker solutions, approximating to $\cdot 432 - \cdot 284 = + \cdot 148$ volt for $0 \cdot 1 \text{ FeSO}_4 \cdot 100 \text{ H}_2\text{O}$.

Hence iron is an example of a class of metals opposite in thermovoltaic power to silver and lead, and differing from copper and cadmium in that, whilst with the last two metals the thermovoltaic constant has never a value largely above or below zero, with silver and lead it has considerable negative values, and with iron it has a notable positive value.

One result of this is that a zinc-iron-sulphate cell is analogous to a copper-lead-sulphate cell, in that it affords a considerably higher effective E.M.F. than corresponds with the net chemical action, viz. from $\cdot 398$ to $\cdot 432$ instead of $\cdot 284$.

Another result is that, since the thermovoltaic constants for cadmium and iron (in solutions $1 \cdot 0 \text{ MSO}_4 \cdot 100 \text{ H}_2\text{O}$) are respectively $+ \cdot 001$ and $+ \cdot 114$, if iron were to displace cadmium from solution of this strength, there would be evolved $93200 - (89880 - 75) = 3395$ gramme-degrees per gramme-molecule (75 being the heat of dilution from $1 \cdot 0 \text{ CdSO}_4 \cdot 100 \text{ H}_2\text{O}$ to $\cdot 25 \text{ CdSO}_4 \cdot 100 \text{ H}_2\text{O}$), corresponding with a value

for E_H of $\cdot 075$ volt, cadmium acquiring the higher potential ; but upon this chemical source of potential-difference there is superposed the thermovoltaic action

$$k_1 - k_2 = + \cdot 001 - \cdot 114 = - \cdot 113 ;$$

hence the net effective E.M.F. is

$$+ \cdot 075 - \cdot 113 = - \cdot 038 :$$

that is, *instead of cadmium acquiring the higher potential, iron does so*, and a current flows with an E.M.F. of $- \cdot 038$ volt (mean observed value $= - \cdot 035$ volt).

As regards iron-lead-sulphate cells, it is to be remarked that, since lead acquires the higher potential and since the thermovoltaic constant of lead is a considerable negative quantity whilst that of iron is also a considerable positive quantity, the value of $k_1 - k_2$ is largely negative, viz. for solutions of strengths $1 \cdot 0 \text{ MSO}_4 \cdot 100 \text{ H}_2\text{O}$ (zinc-sulphate solution being used to suspend lead sulphate in), $- \cdot 200 - \cdot 114 = - \cdot 314$. That is to say, whilst the value of E_H corresponding to this displacement of lead from lead sulphate (solid) by iron is $\cdot 428$ volt (corresponding with $93200 - 73800 = 19400$ gramme-degrees per gramme-molecule), the effective E.M.F. of the cell is little more than one fourth of this, viz. $\cdot 428 - \cdot 314 = \cdot 114$ (mean observed value $= \cdot 112$ volt).

The same kind of remark applies to iron-copper-sulphate cells in a less degree, the value of $k_1 - k_2$ being here (for $1 \cdot 0 \text{ MSO}_4 \cdot 100 \text{ H}_2\text{O}$) $+ \cdot 009 - \cdot 114 = - \cdot 105$; that is, whilst the value of E_H is $\cdot 821$ (corresponding with $93200 - 55960 = 37240$ gramme-degrees per gramme-molecule), the effective E.M.F. of the cell is only $\cdot 821 - \cdot 105 = \cdot 716$ (mean observed value $= \cdot 715$ volt).

It should consequently result that, if a cell be set up with an electro-silver plate immersed in saturated silver-sulphate solution opposed to bright iron in an equally strong ferrous-sulphate solution, viz. $\cdot 042 \text{ FeSO}_4 \cdot 100 \text{ H}_2\text{O}$, the value of $k_1 - k_2$ will be largely negative, approximating to $- \cdot 351 - \cdot 148 = - \cdot 499$; so that as E_H is, in this case, $1 \cdot 605$ (corresponding with $93200 - 20390 = 72810$ gramme-degrees per gramme-molecule), the effective E.M.F. will be near to $1 \cdot 605 - \cdot 499 = 1 \cdot 106$ volt. In point of fact, a number of such cells gave on examination the average value $1 \cdot 103$ with a probable error of $\pm \cdot 003$ volt.

Effect of varying the Plate-surface in Iron-copper-sulphate Cells ; and Rate of Depreciation of E.M.F. of such Cells with increasing Current-density.

176. Some experiments were carried out precisely as described in §§ 115, 124, and 131, with cells set up with polished

iron plates immersed in a strong solution of ferrous sulphate opposed to electro-copper plates in copper-sulphate solution. The values obtained show that, whilst with zinc-copper, zinc-cadmium, cadmium-copper, zinc-silver, and copper-silver cells halving the area of the plate on which metal is deposited usually causes a greater decrease in the E.M.F. for a given rate of current-flow than is caused by halving the area of the other plate, the reverse is the case with iron-copper cells. Thus, for example, in one experiment the following numbers were obtained, and similarly in other cases:—

Current in micro-ampères.	Effect of halving area of the	
	Iron plate.	Copper plate.
500	·008	·005
1000	·010	·006
2000	·012	·008
5000	·017	·012
10,000	·027	·021

With iron-copper-sulphate cells the rate at which the E.M.F. of the cell diminishes as the current-density increases is distinctly more rapid than with cadmium-copper-sulphate cells, or with Daniell cells (zinc-copper-sulphate): thus, for instance:—

Rate of current-flow in microampères per square centimetre.	Average E.M.F.	Fall in E.M.F.
0	·711	
20	·708	·003
40	·698	·013
100	·685	·026
200	·671	·040
400	·656	·055
1000	·632	·079
2000	·604	·107

It is noticeable that the substitution of dilute sulphuric acid for ferrous-sulphate solution in an iron-copper cell uniformly tends to *lower* the E.M.F., also rendering the cell-values less permanent and subject to wider ranges of fluctuation than before, probably on account of the “local action” of the acid on the iron; a similar result is also brought about by the substitution of dilute hydrochloric acid for ferrous-chloride solution in cells containing iron and other metals with chloride solutions.

VI. Magnesium.

177. Two sets of cells were examined containing bright magnesium (wire) immersed in magnesium-sulphate solution opposed to amalgamated zinc and electro-copper in their sulphate solutions respectively, the solution-strength being 1.0 MSO_4 100 H_2O throughout.

The galvanometer-readings showed a slight increase in each case as time elapsed after first setting up; the following figures represent the mean readings of each cell during the first half hour after setting up, reduced to volts :—

	Magnesium-Zinc.	Magnesium-Copper.
Maximum mean reading ...	·740	1·854
Minimum „ „ ...	·713	1·827
Average „ „ ...	·724	1·840
Probable error	± 0042	± 0041

Hence the following valuations of the voltaic constant result; these values are negative, since zinc acquires the higher, and magnesium the lower potential :—

Zinc-Magnesium cell -·724

Magnesium-Copper + Zinc-Copper... $\left\{ \begin{array}{l} -1\cdot840 \\ +1\cdot114 \end{array} \right\}$ -·726

Mean = -·725

Julius Thomsen finds Mg , O , SO_3 aq. = 180180 for ·25 MgSO_4 100 H_2O , whence for this strength $E_H = -1\cdot634$. Moreover, he finds practically no difference between the heats of dilution of magnesium and zinc-sulphate solutions of strength 1.0 MSO_4 100 H_2O , so for this strength also E_H sensibly = -1.634. Hence the thermovoltaic constant for bright magnesium in 1.0 MgSO_4 100 H_2O is

$$-0.725 - (-1.634) = +0.909.$$

VII. Aluminium.

178. Two sets of cells were examined, containing bright aluminium plates opposed to amalgamated zinc and electro-copper respectively, immersed in sulphate solutions of strength ·5 MSO_4 100 H_2O (instead of using aluminium sulphate, pure potash alum was employed, the potassium sulphate present

therein being left out of consideration in valuing the molecular strength). From the heat of formation of aluminium-sulphate solution being greater than that of zinc-sulphate, it might naturally be expected that zinc would acquire the higher potential, as with magnesium-zinc cells; but this was not the case, aluminium-zinc-sulphate cells resembling cadmium-iron-sulphate cells (§ 175) in that the current actually generated flows in the direction opposite to that predicable from the heats of formation of the electrolytes: for this reason the cell-values are marked with the — sign. The following figures represent the mean readings during the first half hour after setting up: with aluminium-zinc cells the readings slightly diminished in numerical value as time elapsed, and *vice versa* with the copper-aluminium cells:—

	Zinc-Aluminium.	Aluminium-Copper.
Maximum mean reading ...	—·548	+·588
Minimum „ „ ...	—·526	+·566
Average „ „ „	—·538	+·578
Probable error	±·0034	±·0030

From these figures the following valuations of the voltaic constant result; this constant is of + sign, since aluminium acquires the higher, and zinc the lower potential:—

Zinc-Aluminium +·538

Zinc-Copper—Aluminium-Copper $\left\{ \begin{array}{c} 1\cdot114 \\ -\cdot578 \end{array} \right\} = +\cdot536$

Mean = +·537

Julius Thomsen finds that $\text{Al}_2\text{O}_3, \text{SO}_3 \text{ aq.} = 150630$; whence $E_H = -\cdot982$ volt. This value is probably not strictly applicable in the case of the cells now under discussion (which contained potash alum and not aluminium sulphate); but the error introduced by taking this value is but small as compared with the thermovoltaic constant deduced, viz.

$$\cdot537 - (-\cdot982) = +1\cdot519.$$

VIII. Mercurous-Zinc-sulphate Cells.

179. A number of cells were examined consisting of amalgamated-zinc plates immersed in zinc-sulphate solution on the

one side, and of pure mercury with a magma of mercurous-sulphate and zinc-sulphate solution on the other, the zinc sulphate being of the same strength on each side : with these cells the E.M.F. continually fell as the solution-strength rose*. It is evident that these cells differ but little from a cold-prepared Clark's cell, the essential difference being that in the latter the zinc rod is not coated with fluid mercury to begin with, and is immersed, not in pure zinc-sulphate solution, but in the same magma of mercurous-sulphate and zinc-sulphate solution as surrounds the mercury. This difference in construction gives rise to a slight difference in E.M.F. with weaker solutions of zinc sulphate, the Clark's cell uniformly reading lowest ; with saturated or very strong solutions the difference between the two kinds of cell is less than the experimental errors. Thus the following average values were obtained as the voltaic constants of mercury in contact with mercurous sulphate and m ZnSO_4 100 H_2O , and as the E.M.F.'s of cells like Clark's but set up with m ZnSO_4 100 H_2O instead of saturated zinc-sulphate solution :—

m .	Voltaic constant.	E.M.F. of Clark's cell.
	Volt.	Volt.
5.75	1.457	1.457
4.89	1.462	1.462
4.33	1.466	1.465
3.58	1.471	1.469
2.7	1.477	1.475
.1	1.514	1.510

The heat of formation of mercurous sulphate being unknown, the corresponding thermovoltaic constants cannot be exactly calculated ; in all probability, however, the value of E_H is near to 1.202 volt, the value for mercurous-zinc-nitrate cells, when $m = .25$ (§ 192), so that the thermovoltaic constant is a large positive quantity, near to $+.25$ volt at least.

[To be continued.]

* It may be noticed that mercurous sulphate is about three times as soluble in saturated zinc-sulphate solution as in distilled water. Three saturated solutions gave, on analysis, numbers represented by the following formulæ :—

Distilled water. $.0017 \text{ Hg}_2\text{SO}_4$ 100 H_2O .
 Zinc-sulphate solution, about one-
 third saturated. $.0042 \text{ Hg}_2\text{SO}_4$, 2.0 ZnSO_4 , 100 H_2O .
 Saturated zinc-sulphate solution . . $.0054 \text{ Hg}_2\text{SO}_4$, 5.75 ZnSO_4 , 100 H_2O .

II. *On Arctic Interglacial Periods.*
*By JAMES CROLL, LL.D., F.R.S.**

P*OLAR Interglacial Periods more marked than the Glacial.*
 —In a former paper †, and also in ‘Climate and Time’ (chap. xvi.), it was pointed out that in temperate regions the cold periods of the glacial epoch would be far more marked than the warm interglacial periods. In temperate regions the condition of things which prevailed during the cold periods would differ far more widely from that which now prevails than would the condition of things during the warm periods. But as regards the polar regions the reverse would be the case; there the warm interglacial periods would be more marked than the cold periods. The condition of things prevailing in these regions during the warm periods would be in strongest contrast to what now obtains; but this would not hold true in reference to the cold periods, during which matters would be pretty much the same as at present, only somewhat more severe. In short, the glacial state is the normal condition of the polar regions, the interglacial the abnormal. At present Greenland and other parts of the Arctic regions are almost wholly covered with snow and ice, and, consequently, nearly destitute of vegetable life. In fact, as regards organic life in those regions, matters during the glacial epoch would not probably be much worse than they are at the present day. Greenland and the Antarctic continent are to-day almost as destitute of plant-life as they could possibly be. Although, in opposition to what is found to be true in reference to the temperate regions, the polar interglacial periods were more marked than the glacial, it does not follow that on this account the relics of the interglacial periods which remain ought to be more abundant in polar than in temperate regions. On the contrary, the reverse ought to be the case. In the polar regions, undoubtedly, there is least likelihood of finding traces of interglacial periods; for there, of all other places, the destruction of such traces would be most complete. The more severe the glaciation following a warm period, the more complete would be the removal of the remains belonging to the period. If in such places as Scotland and Scandinavia so little is left of the wreck of interglacial periods, it need be a matter of no surprise that in Arctic regions scarcely a relic of those periods remains. The comparative absence in polar regions of

* Communicated by the Author.

† Phil. Mag. May 1884, p. 375; American Journal of Science, June 1884.

organic remains belonging to a mild interglacial period cannot therefore be adduced as evidence against the probable existence of such a period. Who would expect to find such remains in ice-covered regions like Greenland and Spitzbergen? Although not a trace is now to be found, it is nevertheless quite possible that during interglacial periods those regions may have enjoyed a comparatively mild and equable climate.

Evidence from the Mammoth in Siberia.—This comparative absence of the remains of a warmer condition of climate in Arctic regions during Pleistocene times holds true, however, only in regard to those parts, like Greenland, which have undergone severe glaciation. When we examine Siberia and other places which appear to have escaped the destructive power of the ice, we find, from a class of facts the physical importance of which appears to have been greatly overlooked, abundant proofs of a mild and equable condition of climate. I refer to facts connected with the climatic condition under which the Siberian Mammoth and his congeners lived. The simple fact that the Mammoth lived in Northern Siberia proves that at the time the climate of that region must have been far different from what it is at the present day.

The opinion was long held, and is still held by some, that the Mammoth did not live in Northern Siberia, where his remains are found, but in more southern latitudes, and that these remains were carried down by rivers. It was considered incredible that an animal allied to the Elephant, which now lives only in tropical regions, should have existed under a climate so rigorous as that of Siberia. The opinion that the remains were floated down the Siberian rivers is now, however, abandoned by Russian naturalists and other observers who have carefully examined the country.

I shall here give a brief statement of the facts and arguments which have been adduced in support of the theory that the Mammoth lived and died where its remains were found. For these facts I am mainly indebted to the admirable papers by Mr. Howorth on the "Mammoth in Siberia," which appeared in the 'Geological Magazine' for 1880.

Had the remains of the Mammoth been carried down from the far south by the Siberian rivers, they would have been found mainly, if not exclusively, on the banks of the long rivers, such as the Obi, Yenissei, and the Lena, and in the deltas formed at their mouths. But such is not the case. "These are," says Mr. Howorth, "found even more abundantly on the banks of the very short rivers east of the Lena. They are found not only on the deltas of these rivers, but far

away to the north, in the islands of New Siberia, beyond the reach of the currents of the small rivers, whose mouths are opposite those islands." But a more convincing proof is that "they are found not only in North Central Siberia, where the main arteries of the country flow, but in great numbers east of the river Lena, in the vast peninsula of the Chukchi, in the country of the Yukagirs, and in Kamtskatka, where there are no rivers down which they could have floated from more temperate regions." Besides, it is not merely in the deltas and banks of rivers that the remains are found, but in nearly all parts of the open tundra; and Wrangell says* that the best as well as the greatest number of remains are found at a certain depth below the surface in clay-hills, and more in those of some elevation than along the low coast or in the flat tundra.

Had the Mammoth lived in the south we should, as Mr. Howorth further remarks, have found its remains most abundant in the south, whereas the further north we go the remains become more abundant, and in the islands of the Liachof archipelago, in about latitude 74° , the greatest quantities have been discovered. Again, according to Hedenstrom, the bones and tusks found in the north are not so large and heavy as those in the south; a fact which still further confirms the opinion that the Mammoth lived where his remains are found, inasmuch as the greater severity of the climate in northern parts would certainly hinder the growth and full development of the animal.

Northern Siberia much Warmer during the Mammoth Epoch than now.—It is true that the Mammoth and the *Rhinoceros tichorhinus* were furnished with a woolly covering which would protect them from cold; but it is nevertheless highly improbable that they could have endured a climate so severe as that of Northern Siberia at the present day, where the ground is covered with snow for nine months in the year and the temperature is seldom much above zero Fahr. And even if they could have endured the cold, they would have starved for want of food. Some parts of Siberia are no doubt fertile, as, for example, the valley of the Yenisei, described by Nordenskjöld†; but there is little doubt, as Mr. Howorth remarks, that the larger portion of Northern Siberia, where the Mammoth and the *Rhinoceros* lived, is now a naked tundra covered with moss, on which no tree will grow. On such ground it is physically impossible that the Mammoth and *Rhinoceros* could exist, for they cannot graze close to

* 'Polar Sea Expedition,' English translation, p. 275.

† 'Nature,' Dec. 2, 1875.

the ground like oxen. They live on long grass and on the foliage and small branches of trees.

Evidence from Wood.—The fact that the Mammoth was most abundant beyond the present northern limit of wood is pretty good evidence that the climatic condition of Northern Siberia must have been milder than now. Wood must have extended, in the days of the Mammoth, far beyond its present limit, probably as far north as New Siberia : facts of observation support this conclusion.

The wood found in Northern Siberia consists of two classes—the one is the result of drift, the other grew on the spot. The natives call the former “Noashina,” and the latter “Adamshina;” and the division is supported by Göppert, “who separates the trunks of timber found in Northern Siberia into a northern series, with narrow rings of annual growth, and a southern, with wider ones. The latter doubtless floated down the rivers, as great quantities do still; while the former probably grew here with the Mammoth.”

In the middle of October 1810 Hedenstrom went across the tundra direct to Ustiansk. “On this occasion,” he says, “I observed a remarkable natural phenomenon on the Chastach Lake. This lake is 14 versts long and 6 broad, and every autumn throws up a quantity of bituminous fragments of wood, with which its shores in many places are covered to the depth of more than 2 feet. Among these are pieces of a hard transparent resinous substance, burning like amber, though without its agreeable perfume. It is probably the hardened resin of the larch tree. The Chastach Lake is situated 115 versts from the sea and 80 versts from the nearest forest.” *

On the same journey Hedenstrom noticed “on the tundra, equally remote from the present line of forest, among the steep sandy banks of the lakes and rivers, large birch trees, complete, with bark, branches, and root. At the first glance they appeared to have been well preserved by the earth; but, on digging them up, they are found to be in a thorough state of decay. On being lighted they glow, but never emit a flame: nevertheless the inhabitants of the neighbourhood use them as fuel, and designate these subterranean trees as *Adamoushtshina*, or of Adam’s time. The first living birch tree is not found nearer than three degrees to the south, and then only in the form of a shrub.” †

On the hills in the interior of the island of Koteloni “Sannikow found the skulls and bones of horses, buffaloes,

* Wrangell, p. 491.

† Ibid. p. 492.

oxen, and sheep in such abundance that these animals must formerly have lived there in large herds. At present, however, the icy wilderness produces nothing that could afford them nourishment, nor would they be able to endure the climate. Sannikow concludes that a milder climate must formerly have prevailed here, and that these animals may therefore have been contemporary with the Mammoth, whose remains are found in every part of the island." *

"Herr von Ruprecht reported to Brandt that, at the mouth of the Indiga, in $67^{\circ} 39'$ N. lat., on a small peninsula called Chernoi Noss, where at present only very small birch bushes grow, he found rotten birch trunks still standing upright, of the thickness of a man's leg and the height of a man. In going up the river he met with no traces of wood until he reached the port of Indiga. Here he noticed the first light-fir woods growing among still standing but dead trunks. And higher up the river still, the living woods fairly began." †

Schmidt says that, "where the lakes on the tundra have grown small and shallow, we find on and near their banks a layer of turf, under which, in many places, *are remains of trees in good condition, which support the other proofs that the northern limit of trees has retrogressed, and that the climate here has grown colder.* I found, on the way from Dudino to the Ural Mountains, in a place where larches now only grow in sheltered river-valleys, in turf on the top of the tundra, prostrate larch trees still bearing cones." ‡

Schmidt also states that he was informed that at Dudino, just at the limit of the woods, there had been found in a miserable larch wood the lower part of a stem sticking in the ground, apparently rooted, which was three feet in diameter. He also states that, "eleven versts above Krestowkoje, in lat. 72° , he found, in a layer of soil covered with clay on the upper edge of the banks of the Yenissei, well-preserved stems like those of the birch, with their bark intact, and sometimes with their roots attached, and three to four inches in diameter. Professor Merklin recognizes them as those of the *Alnaster fruticosus*, which still grows as a bush on the islands of the Yenissei, in lat. $70\frac{1}{2}^{\circ}$ N."

Evidence from Shells.—In the freshwater deposits in which the bones of the Mammoth are found, there are freshwater- and land-shells, which indicate a warmer condition of climate. I quote the following from Mr. Howorth's memoir:—

"Schmidt found *Helix schrencki* in freshwater deposits on

* Wrangell, p. 496.

† Bull. of Soc. of Nat. of Moscow; quoted by Howorth.

‡ Schmidt, as quoted by Howorth.

the tundra below Dudino and beyond the present range of trees. Lopatim found recent shells of it, with well-preserved colours, 9° further south, in lat. 68° and 69°, within the present range of trees, at the mouth of the Awamka. The most northern limit hitherto known for this shell was in lat. 60° N., where they were found by Maak in gold-washings on the Pit."

"In the freshwater clay of the tundra by Tolstoi Noss, Schmidt found *Planorbis albus*, *Valvata cristata*, and *Limnæa auricularia* in a subfossil state; *Cyclas calyculata* and *Valvata piscinalis* he found thrown up on the banks of the Yenissei, and on a rotten drifted trunk, *Limax agrestis*; *Anodonta anatina* he also found on the banks of the Yenissei as far as Tolstoi Noss, but no further. *Pisidium fontinale* still lives in the pools on the tundra; as does *Succinea putris* on the branches of the Alnaster on the Brijochof Islands."

Mr. Belt mentions* that the *Cyrena fluminalis* is found in Siberia in the same deposits which contain the remains of the Mammoth and the *Rhinoceros tichorhinus*.

"The evidence, then," says Mr. Howorth, "of the *débris* of vegetation, and of the freshwater- and land-shells found with the Mammoth-remains, amply confirms the *à priori* conclusion that the climate of Northern Siberia was at the epoch of the Mammoth much more temperate than now. It seems that the botanical facies of the district was not unlike that of Southern Siberia, that the larch, the willow, and the Alnaster were probably the prevailing trees, that the limit of woods extended far to the north of its present range and doubtless as far as the Arctic Sea; that not only the mean temperature was much higher, but it is probable that the winters were of a temperate and not of an Arctic type." (Geol. Mag. Dec. 1880.)

The Mammoth Interglacial.—It need be a matter of no surprise that the climate of Northern Siberia during the time of the Mammoth was more mild and equable than now, if we only admit that the Mammoth was interglacial. That it was of interglacial age is a conclusion which, I think, has been well established by Prof. J. Geikie and others. Into the facts and arguments which have been advanced in support of this conclusion I need not here enter. The subject will, however, be found discussed at great length in Prof. Geikie's 'Prehistoric Europe' and in 'The Great Ice-Age' (second edition). Mr. R. A. Wallace considers that one of the last intercalated mild periods of the glacial epoch seems to offer all the necessary conditions for the existence of the Mammoth in Siberia. That the Mammoth was interglacial will be further evident when we consider the climatic conditions of Europe at the

* Quart. Journ. Geol. Soc. vol. xxx. p 464.

time that it lived there. Before doing so, it may be as well to glance at what evidently were the main characteristics of the interglacial periods.

Main Characteristics of Interglacial Climate.—They are as follows :—

1. Interglacial conditions neither did nor could exist *simultaneously* on both hemispheres. They existed only on one hemisphere at a time, viz. on the hemisphere which had its winter solstice in perihelion.

2. During interglacial periods the climate was more *equable* than it is at present ; that is to say, the difference between the summer and winter temperatures was much less than it is now. The summers may not have been warmer or even so warm as they are at present, but the temperature of the winters was much above what it is at the present day.

3. During interglacial periods the quantity of equatorial heat conveyed by ocean-currents into temperate and polar regions was far in excess of what it is at present. On this account a greater *uniformity* of climate then prevailed : that is to say, the difference of climatic conditions between the subtropical and the temperate and polar regions was less marked than at present—the temperature not differing so much with latitude as it now does.

4. *Mildness*, or a comparative absence of high winds, characterized interglacial climate. This partial exemption from high winds resulted from the fact that the difference of temperature between the equator and the poles, the primary cause of the winds, was much less than at the present day.

5. Another character of interglacial climate was a *higher mean temperature* than now prevails. This, amongst other causes, resulted from the great amount of heat then transferred by ocean-currents from the glacial to the interglacial hemisphere.

6. During interglacial periods the climate was not only more equable, mild, and uniform than now, but it was also more *moist*. This was doubtless owing mainly to the fact of the presence then in temperate and polar regions of so large an amount of warm intertropical water. In short, it was the presence of so much warm water from intertropical regions which mainly gave to the climate of the interglacial periods its peculiar character.

All these characteristics of interglacial climate have been fully established by the facts of geology, but they are also, as we have seen, deducible *à priori* from physical principles. They follow as *necessary consequences* from those physical agencies which brought about the glacial epoch.

Evidence from the Mammoth in Europe.—Skeletons and detached remains of the Mammoth have been found in nearly every country in Europe. Mr. Howorth, in his memoir*, gives the details of the finding of these in various parts of Russia, Germany, Denmark, Sweden, Belgium, France, England, and other countries. It is shown that the conditions under which the Mammoth-remains have been found in Europe are almost identically the same as those under which they are found in Siberia, with the exception, of course, that in Europe no carcasses with their flesh intact have been met with.

Again, the deposit in which the Mammoth-remains are found in Europe is the same as that in which they occur in Siberia. The deposit is a freshwater one, consisting of marly clay and gravel, and containing plant-remains and land- and freshwater-shells. When these plants and shells are examined, they are found to indicate the same interglacial condition of climate as that which prevailed in Siberia during the time the Mammoth lived in that region.

In the case of land-plants it is, of course, only under exceptional circumstances, as Prof. J. Geikie remarks, that they can be found in a condition suitable for the botanist. Now and again, however, beds with well-preserved plants are met with, buried under lacustrine deposits. In a still better state of preservation are the plant-remains and shells which have been discovered in the masses of calcareous tufa which have been formed upon the borders of incrustating springs. An examination of the plant-remains found under those conditions shows that during Pleistocene times, when the deposits in which the Mammoth bones are found were being formed, the climate was more equable and uniform than it is at the present day.

The fossiliferous remains yielded by the tufas have led to most important results as to the climatic condition of the Pleistocene period, into the details of which I need not here enter. These will be found at full length in Prof. J. Geikie's 'Prehistoric Europe,' chap. iv.† It will suffice at present simply to refer to the general conclusions to which these researches have led, in so far as they bear on the climatic conditions prevailing at the time the Mammoth lived so abundantly in Europe.

In the tufa deposits of Tuscany have been found numbers of plant-remains of indigenous species, commingled with others which now no longer grow in Tuscany. Amongst the latter is the Canary laurel, which now flourishes so luxuriantly in the Canary Islands, on the northern slopes of the mountains,

* Geol. Mag. May 1881.

† See also Mr. Howorth's memoir, Geol. Mag. June 1881.

at an elevation of from 2000 to 5000 feet above the sea-level—a region, remarks Prof. J. Geikie, nearly always enveloped in steaming vapours, and exposed to heavy rains in winter. In that deposit is also found the common laurel, associated with the beech. This is not now the case, as the laurel requires more shade than it can find there at present, while the beech has retreated to the northern flanks of the Apennines to obtain a cooler climate.

In the tufas of Provence are found groups the same as those which flourish there at present, but commingled with them are also the Canary laurel and other plants which are no longer natives of Provence. Saporta directs attention to the fact that species such as the Aleppo pine and the olive, demanding considerable summer-heat rather than a moist climate, are entirely wanting in the tufas.

Similar to those of Provence are the tufas of Montpellier. Saporta concludes that when all those species lived together the climate must necessarily have been *more equable and humid* than at present. In other words, the summers were not so dry and the winters were milder than they are now.

The deposit near Moret, in the valley of the Seine, is still more remarkable in showing the equable condition of climate which then prevailed. The assemblage of plants found there tells a tale, says Prof. J. Geikie, which there is no possibility of misreading. “Here,” he says, “we have the clearest evidence of a genial, humid, and equable climate having formerly characterized Northern France. The presence of the laurel, and that variety of it which is most susceptible to cold, shows us that the winters must have been mild, for this plant flowers during that season, and repeated frosts, says Saporta, would prevent it reproducing its kind. It is a mild winter rather than a hot summer which the laurel demands, and the same may be said of the fig-tree. The olive, on the other hand, requires prolonged summer heat to enable it to perform its vital functions. Saporta describes the fig-tree of the La-Celle tufa as closely approximating, in the size and shape of its leaves and fruit, to that of the tufas in the south of France, and to those of Asia Minor, Kurdistan, and Armenia. But if the winters in Northern France were formerly mild and genial, the summers were certainly more humid, and probably not so hot. This is proved by the presence of several plants in the tufa of La Celle which cannot endure a hot arid climate, but abound in the shady woods of Northern France and Germany.”

The plants found in the tufas of Canstadt are much similar to those of Moret. Mr. Howorth, in regard to the deposits of

those places, says:—"The coexistence of the species found there, remarks M. Saporta, proves very clearly that, notwithstanding the variations due to latitude, Europe, from the Mediterranean to its central districts, offered fewer contrasts, and was more uniform than it is now. A more equable climate, damp and clement, allowed the *Acer pseudo-platanus* and the fig to live associated together near Paris, as it allowed the reindeer and hyæna. The *Acer* grows with difficulty now where the *Ficus* grows wild, while the latter has to be protected in winter in the latitude of Paris."*

Equally conclusive is the testimony borne by the Mollusca of the tufas. In the tufas and marls of Moret, in the valley of the Seine, thirty-five species were discovered. The majority of these must have lived in damp and shady places, in the recesses of moist woods, and on the leaves of marsh-plants. The shells, M. Tournouër concludes, bespeak a condition of climate more uniform, damp, and equable than now prevails in that region, with a somewhat higher mean annual temperature. In the alluvial deposits of Canstadt, in Würtemberg, a class of shells indicating a similar condition of climate has been discovered.

The evidence furnished by the animals found most abundantly with the Mammoth in Europe and Siberia, Mr. Howorth thinks, points to the same conclusion as that of the plants and mollusca.

The same mild and equable condition which allowed of the Mammoth living in Northern Siberia during Pleistocene times thus equally prevailed over the whole of Europe. We have seen that, according to the Physical Theory, this condition of climate was in every respect precisely what it ought to have been on the supposition that it was interglacial. It was a condition mild, equable, uniform, humid, and of a higher mean annual temperature than we have at the present day. There is, however, direct and positive evidence that this condition of climate was interglacial; for the facts both of geology and of palæontology show that it was preceded and succeeded by a state of things of a wholly opposite character.

The Mammoth Glacial as well as Interglacial.—Although the Mammoth could have lived in Arctic Siberia only during an interglacial period, it does not follow that it must have perished during the succeeding glacial period. When the cold came on, and the vegetation on which it subsisted began to disappear, it would move southwards, and would continue its march as the cold and severity of the winters increased.

* Geol. Mag. June 1881.

During the continuance of the ten or twelve thousand years of Arctic conditions it would find in Southern Europe and elsewhere places where it could exist. At the end of the cold period, and when the climate again began to grow mild and equable, it would retrace its steps northwards. There is, however, little doubt that during the severity of a glacial period, and when necessarily confined to a more limited area, its numbers would be greatly diminished. There is every reason for believing that the Mammoth outlived all that succession of cold and warm periods known as the glacial epoch proper, and did not finally disappear till recent postglacial times.

It was probably about the commencement of a cold period, and before the Mammoth had retreated from Northern Siberia, that those individuals perished whose carcasses have been found frozen in the cliffs. The way in which they probably perished and became imbedded in the frozen mud and ice, has, I think, been ingeniously shown by Dr. Rae*.

Arctic America during Interglacial times.—We have seen that the eastern continent in Pleistocene times enjoyed in the Arctic regions interglacial conditions of climate. It is true that on the western continent we have not in Arctic regions such clear and satisfactory evidence of an interglacial period. But it would be rash to infer from this that the western continent was, in this respect, less favoured than the eastern. That we should find less evidence at the present day of former interglacial periods in Arctic America than in Arctic Asia, is what is to be expected, for the glaciation which succeeded interglacial periods has been far more severe in the former region than in the latter. The remains of the Mammoth have, however, been found in Arctic America, in ice-cliffs at Kötzebue Sound, under conditions exactly similar to those of Siberia.

In Banks's Land, Prince Patrick's Island, and Melville Island, as in Northern Siberia, full-grown trees have been found in abundance at considerable distances in the interior, and at elevations of two or three hundred feet above sea-level. The bark on many of them was in a perfect state. Capt. McClure, Capt. Osborn, and Lieut. Mecham, by whom they were found, all agreed in thinking that they grew in the place where they were found.

It is true that more recent Arctic voyagers have come to the conclusion that these trees must have been drifted down the rivers from the south. There can be little doubt that the greater part of the wood found there, as in Siberia, is drift-wood. But may there not be also, as in Siberia, two kinds

* Phil. Mag. for July 1874, p. 60.

of wood?—a “Noashina” and an “Adamshina,” a kind which was drifted and another kind which grew on the spot. This is a point which will require to be determined.

That so little has as yet been done in the way of searching for such evidence of interglacial periods, is, doubtless, in a great measure due to the fact that most of those, if not all, who have visited those regions entertained the belief that there is an *à priori* improbability that a condition of climate which would have allowed the growth of trees in such a place prevailed so recently as Post-tertiary times. Even supposing those Arctic voyagers had considered the finding of interglacial deposits a likely thing, and had in addition made special search for them, the simple fact that they should have failed to find any trace of them could not, as we have already shown, be regarded as even presumptive evidence that none existed. Take Scotland as an example. Abundant relics of interglacial age have there been found from time to time; but amongst the many geologists who visit that country year by year, how few of them have the good fortune of discovering a single relic. In fact a geologist might search for months, and yet fail to meet with an interglacial deposit. The reason is obvious. The last ice-sheet, under which Scotland was buried, was so enormous as to remove every remnant of the preceding interglacial land-surface, except here and there in deep and sheltered hollows, or in spots where it may happen to have been protected from the grinding power of the ice by projecting rocks. But all those places are now so completely covered with boulder-clay and other deposits that it is only in the sinking of pits, quarries, in railway-cuttings, and other deep excavations that traces of them accidentally turn up. Now if it is so difficult to find in temperate regions, in a place like Scotland, interglacial remains, how much more difficult must it be to meet with them in Arctic regions, where the destructive power of the ice must have been so much greater.

Something like indications of an interglacial period appear to have been found by Professor Nordenskjöld in Spitzbergen. “In the interior of Ice-fjord,” he says, “and at several other places on the coast of Spitzbergen, one meets with indications either that the polar tracts were less completely covered with ice during the glacial era than is usually supposed, or that, in conformity with what has been observed in Switzerland, interglacial periods have also occurred in the polar regions. In some sandbeds not very much raised above the level of the sea one may, in fact, find the large shells of a mussel (*Mytilus edulis*) still living in the waters encircling the Scandinavian coast. It is now no longer found in the sea around

Spitzbergen, having been probably routed out by the ice-masses constantly driven by the ocean-currents along the coasts.”*

This testimony is the more valuable as it is given by an experienced geologist so much opposed to the theory of interglacial periods. A more special and thorough search of those beds might probably reveal further indications of interglacial age.

Was Greenland free from Ice during any of the Interglacial Periods?—There is nothing whatever improbable in the supposition that during some of the earlier interglacial periods, when the eccentricity was about a maximum, the ice might have completely disappeared from Greenland, and the country become covered with vegetation.

Mr. Wallace thinks that the existence at present of an ice-sheet on Greenland is to be explained only by the fact that cold currents from the polar area flow down both sides of that continent. He further thinks that could these two Arctic currents be diverted from Greenland, “that country would become free from ice, and might even be completely forest-clad and habitable.”†

I am inclined to agree with Mr. Wallace in thinking that the withdrawal of the two cold currents in question would effectually remove the ice. We know that Greenland is at present buried under ice, as has been shown on former occasions, simply because there happens to be about two inches more of ice annually formed than is actually melted. It certainly would not require any very great change in the present physical and climatic conditions of things to melt two additional inches per annum. If this were done the ice would ultimately disappear. A simple decrease in the volume of the two currents might possibly bring about such a result. A cause more effectual would, however, be an increase in the temperature and volume of the Arctic branch of the Gulf-stream.

NOTE.—This will probably be my last paper on questions relating to geological climate. There are many points I should have wished to consider more fully, but advancing years and declining health have rendered it necessary for me to abandon the subject altogether in order to be able to finish some work, in a wholly different field of inquiry, which has been laid aside for upwards of a quarter of a century.

* “On former Climate of Polar regions,” *Geol. Mag.* Nov. 1875, p. 531. See also “Geology of Spitzbergen,” *Geol. Mag.* 1876, p. 267.

† ‘Island Life,’ p. 149.

III. *A Capillary Multiplier.* By A. M. WORTHINGTON, M.A.*

[Plate I. figs. 1 & 2.]

THIS little instrument (Plate I. fig. 1) consists simply of a rectangular strip of platinum foil of known length rolled into a cylindrical coil, whose successive convolutions are kept separate at a distance of about 2 mm. by means of a strip of glass beads in the upper portion of the coil. The lower edges of all the convolutions are in the same plane; and the instrument when in use hangs from one end of a balance-beam with this plane horizontal.

The liquid whose surface-tension is to be determined is placed below the coil in a vessel which can be raised till the free surface touches the base of the coil. The pull now exerted on the coil is measured by weights placed in the other pan; and these weights afford, when certain precautions shortly to be mentioned are taken, an accurate measure of the surface-tension of the liquid.

The method is really that used twenty years ago by Wilhelmy†, and more recently by M. Dupré‡, who, however, employed only a single cylindrical sheet of platinum instead of a coil, and an hydrometer instead of a balance for measuring the pull.

The only novelty lies in the substitution of the coil for the single sheet, by which the sensitiveness of the method is increased many-fold.

The principles involved may be briefly explained as follows: When any liquid is placed in a vessel whose sides it wets, *i. e.* against which the edge-angle of the liquid is 0° , the liquid is raised round the edge above the level of the free surface, and the weight of liquid so raised is equal to the surface-tension of the liquid multiplied by the periphery across which the surface-tension acts. When the periphery (p) is known and the weight (w) of liquid raised is known, then the tension per unit of length is known to be $\frac{w}{p}$.

When a capillary tube is used for measuring the surface-tension, the periphery across which the force acts is the internal perimeter of the tube, and this is too small to be

* Communicated by the Physical Society: read November 22, 1884 (communicated by J. H. Poynting).

† Poggendorff's *Annalen*, 1863, No. 6, p. 177.

‡ See *Théorie mécanique de la Chaleur*, par M. A. Dupré (Paris, 1869), p. 245 *et seq.*

capable of very exact measurement ; while the weight of liquid raised is that of a column whose height can indeed be accurately measured, but whose sectional area is again too small to be very correctly determined.

If a very wide tube be used, the perimeter can be measured with much greater accuracy ; but the weight of liquid raised cannot now be determined in the same way as before, since the volume included between the surface of the meniscus and a horizontal plane touching its lowest point is now an important part of the whole volume raised. But the weight of the liquid raised along both the inside and outside perimeters of the tube is equal to the reactionary pull downwards of the liquid on the tube and may be determined by measuring this latter.

M. Dupré's wide cylinder of platinum foil was simply a tube whose inner and outer perimeters were sensibly equal and accurately known, and the pull across this perimeter could be determined in the way described.

The great advantage of the coil over the single sheet is that the periphery across which the tension acts may be made very great, while the diameter of the coil, and consequently the quantity of liquid required and the area of its exposed surface, remains small.

Thus one of my coils is made of a strip 50 centim. long and presents a perimeter of 1 metre, while its diameter is only 3 centim., and the pull of water on this is about 8 grams.

The beads of which the strip is composed for separating the convolutions are made of hard glass, and strung on fine platinum wire, so that the instrument may be very readily cleaned by heating to a bright heat in the Bunsen flame. The beads should not reach within 2 or 3 centim. of the lower edge of the coil, in order that the liquid which rises between the convolutions may not reach so high as to wet the glass. If the consecutive coils are 2 millim. apart, then water (which is raised higher than any other known liquid) will attain a height of about .8 centim. But since the coil is liable to be dragged down into the liquid, it is well to leave an ample margin.

If the coil dips below the plane surface of the liquid, there is a correction necessary on account of the buoyancy of the liquid. If, on the other hand, the base of the coil is raised above the free surface, there is a traction to correct for, due to the adhesion of the liquid to the horizontal section of the spiral edge. By adjusting the level of the liquid-surface after it has wetted the coil to the level of the base of the coil, when the balance-beam is horizontal, the necessity for these

corrections is avoided. The thickest foil that I use is .0025 centim. thick, and the correction due to an error of adjustment of the surface, amounting to 1 millim., would, in a measure of the surface-tension of alcohol, amount to $\frac{1}{260}$ of the whole ; and it is easy to make the adjustment within much narrower limits than 1 millim.

Method of Use.

In order to prevent the coil from being dragged below the surface, or, on the other hand, separated from it, I find it convenient to fix two stops A and B (see fig. 2), just above and below the beam of the small unmounted balance that I employ : two stout pins stuck in a cork that is held in its place by a rod (as in the figure) serve the purpose very well.

After cleaning the coil in the Bunsen flame, adjusting its base so as to be horizontal by means of the three suspending cords, which are made of silk and are provided with beads through which the silk forms nooses like a tent-rope, and accurately counterpoising, a block C is placed below the right-hand pan at such a height that the pan rests on it when the beam is horizontal. Then the liquid to be tested is placed in a thin beaker, which stands on a small table, whose height can be easily adjusted by a ratchet-screw (the end of an optical lantern with a card on the top does very well). This is raised till the platinum is wetted, when it is at once drawn as far below the surface (4 or 5 millim.) as the position of the stop A will permit. The pan D is now pressed down by hand on the block C and held there, while the level of the liquid surface is adjusted to the base of the coil. Then D is released, C removed, and weights added to restore equilibrium.

It will be observed that the coil is, by the process of putting on the weights, gradually drawn out of the liquid, so that the topmost element of the elevated liquid will everywhere be in contact with a portion of the coil that has been thoroughly wetted, which will with most liquids reduce the contact-angle to zero.

A few points may be usefully mentioned about the construction of the coil. To make the strip of glass beads I use combustion-tubing drawn out to a diameter of about 2 millim. and cut first into lengths of about 20 centim. Out of a number of such pieces, a careful selection is made of those of most uniform thickness ; these are then accurately cut to a length of about 2 centim., and of the beads thus made the strip is put together by passing through each in turn in opposite directions the two ends of a fine platinum wire (size no. 36). The smoothness of the glass strip allows the platinum coil to

slide within it, so that by gentle pressure against a hard plane surface the edges of the convolutions can at any time be very accurately adjusted to the same plane. Other forms might doubtless be given to the multiplier, but the cylindrical coil is the most compact and requires for its use the smallest amount of liquid. The labour of double-threading the long strip of beads is considerable, and I have tried to avoid it by substituting a strip of asbestos cardboard, which would allow the coil to be cleansed by the action of the flame as before. But though in other respects satisfactory, the asbestos very rapidly gains in weight through the absorption of the vapour of many liquids, and cannot therefore be used unless wrapped in very thin platinum foil; but when so wrapped it is troublesome to coil, and does not allow the same ready adjustment of the edges of the coil to one plane.

Clifton College, Bristol,
October 16, 1884.

IV. *Note on a Point in the Theory of Pendent Drops.*

By A. M. WORTHINGTON, M.A. *

[Plate I. figs. 3-5.]

IN a paper on Pendent Drops (Proc. Roy. Soc. no. 214, 1881) I explained how, from a tracing of the outline of a drop pendent from a circular base, the value of the surface-tension of the liquid could be deduced with an accuracy that compared favourably with that of any other method.

The most difficult part of the process is the measurement at various levels of the inclination of the tangent of the curve to the axis; and I find that this difficulty does not disappear, as I hoped it might, when a photograph of the magnified image of the drop is substituted for a tracing made by hand.

It is essential to the success of the method to measure this inclination at levels where the horizontal sectional area of the drop is widely different. Thus in a drop shaped as in fig. 3 Plate I. it would be desirable to measure the inclination, say, at the level AB and again at the level CD. Now at AB, where the inclination changes very slowly, its value may be very accurately determined; but at CD the change of inclination is rapid and its determination difficult.

It has lately occurred to me that the necessity of finding the value of the inclination at more than one level may be

* Communicated by the Physical Society: read November 22, 1884 (communicated by J. H. Poynting).

avoided, while that level may be so chosen that the tangent is there vertical or changes its inclination very slowly.

Let ADCE (fig. 4) represent a horizontal circular section of the pendent drop taken at any level; ABC the generating curve in the plane of the paper; DBE the same curve in a plane at right angles to the paper. Consider first the equilibrium as regards vertical forces of the mass of liquid below the plane section. We may equate the vertical component of the surface-tension (T) round the circumference ADCE, to the weight (W) of the liquid below the section + the pressure on the area ADCE; so that, signifying by x the unknown distance of this section from the level of the free surface of the liquid, and by Δ the weight of unit volume of the liquid, we have

$$T \cos \theta \times \pi \overline{AC} = W + \pi \overline{AO}^2 x \Delta. \quad . \quad . \quad . \quad (i.)$$

Again, if we consider the equilibrium, as regards horizontal forces parallel to the plane of the paper, of the mass ABDOE, we see that the surface-tension acting horizontally round the periphery DBE is equal to the hydrostatic pressure on the area DBE + the sum of the tensions across each element of the semicircumference DAE, each resolved first horizontally and then normally to DE. Now the hydrostatic pressure is equal to the area DBE \times density of liquid \times depth of the centre of gravity G of the area below the level of the free surface, and this depth = OG + x ; and I have found that OG can be determined with great accuracy by cutting the figure out of carefully selected writing-paper, and balancing; again the sum of the horizontal tensions across the semicircumference resolved normally to DE = $T \sin \theta \overline{DE}$; so that

$$T \times \text{length DBE} = T \sin \theta \overline{DE} + \text{area DBE} (OG + x) \Delta,$$

or

$$T(\text{length ABC} - AC \sin \theta) = \text{area ABC} (OG + x) \Delta; \quad (ii.)$$

and from these two equations x can be eliminated and T found.

Now the section can be chosen at a place of contrary flexure, where the value of $\cos \theta$ can be obtained with very great accuracy; or, again, if the circular base from which the drop depends be small enough the drop will assume the form shown in fig. 5, in which at a certain level MN the curve is vertical, and $\sin \theta = 0$. When this is the case equation (i.) becomes

$$T \times \pi AC = W + \pi AO^2 x \Delta, \quad . \quad . \quad . \quad (iii.)$$

while (ii.) becomes

$$T \times ABC = \text{area } ABC(OG + x) \Delta. \quad \dots \quad (\text{iv.})$$

The method here shown of considering separately the equilibrium first of vertical and then of horizontal forces is equally applicable to sessile drops and to portions of liquid raised by adhesion to a base, and brings out very clearly many results which have hitherto usually been obtained as special cases by rather complicated processes of integration.

Clifton College, Oct. 27, 1884.

V. On a new Form of Monochord.

By Dr. A. ELSASS of Marburg*.

THE monochord has been employed from time immemorial to demonstrate the laws of transverse vibrations. It is used to show that, in producing its fundamental note, a string vibrates in its whole length, but that, in producing the higher partial tones, nodes are formed; and by its means the law of Mersenne can be demonstrated, according to which the fundamental note of a string depends upon its length, its tension, and its mass. In recent times Melde's apparatus has also been employed to show the mode of vibration of the string, which by its means may be made evident to a large audience.

The essential parts of this apparatus are a tuning-fork and a stretched yielding thread, one end of which is attached to the tuning-fork. If the tuning-fork is made to sound, its vibratory motion induces a transversal vibration of the thread, both when the motion of the tuning-fork takes place at right angles to the thread and when it takes place in the direction of its length and the magnitude of the vibrations of the thread is so great that they can be observed from some distance off. The present communication describes a new form of vibration-apparatus which combines the advantages of the monochord and of Melde's apparatus.

In Melde's apparatus the thread is made to assume different forms of vibration by altering its tension, and with it the ratio of its oscillation-period to that of the body which produces the motion; but the oscillation-period itself, which depends upon the period of the exciting body, is not subject to variation.

In the arrangement to be described, however, all variations can be obtained with the same thread by altering the period of the exciting body, so that the law connecting the normal

* Translated from the *Zeitschrift für Instrumentenkunde* for October 1884, from a separate impression communicated by the Author.

vibration-form of the thread and the period of corresponding number of vibrations can be at once exhibited.

The problem, how to excite a thread to vibration by an external periodic action the period of which can be varied at pleasure, can only be solved by employing a motion of rotation to excite the vibration. If one end of the thread is to be used as the exciting-point, then, in order that the external motion shall take place at right angles to the thread, this point must be fastened in such a way that it cannot be displaced in the direction of the thread, but is capable of motion at right angles to it. This is nearly the case if the thread is attached to a rigid lever capable of motion, without much friction, on fixed points round an axis at right angles to the thread. If, then, by means of a revolving toothed wheel or some similar arrangement, periodical impulses can be given, this arrangement will render possible the solution of the problem proposed.

As far as the nature of the vibrations of the thread thus produced is concerned, it is clear that stationary vibrations of the thread can only take place when the period of the external motion stands in some simple ratio to the period of a component of the natural vibrations of the thread—if, at least, we make the assumption that the lever comes into contact with the rotating body only during an indefinitely small time in each impulse.

We see at once that the vibrations of a thread of which one end is not fixed but takes part in the vibration cannot take place in the same manner as the free vibrations of a thread of which both ends are fixed. The mathematical theory, however, which I intend to give in another place, shows that the difference between the form of the constrained vibrations with which we are concerned and that of the free vibrations of the thread is only of small amount. In the constrained vibrations the thread swings without true nodes, *i. e.* points which are absolutely at rest; but instead it has apparent nodes which are points of minimum amplitude of vibration, and the moving end of the thread itself is such a node of minimum amplitude. This remark, of course, holds good only upon the assumption that the end of the thread attached to the lever makes only very small excursions.

It is easy to take account of the relation which must exist between the period of the external excitation and the period of the vibrations of the thread. Let us imagine that the oscillations of a pendulum are maintained by periodical blows, taking place always in the same direction. Let a and b (fig. 1) denote the limiting positions

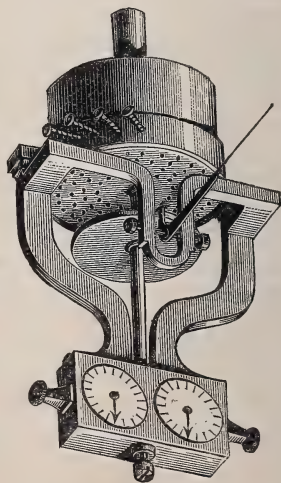
Fig. 1.



of the bob of a pendulum; then it is only necessary each time that it comes into the position *a* to give it a blow in the direction *ab*. The motion will be sustained if the period *T* of the excitation is equal to the period of the oscillation. We see, however, that the motion of the pendulum may also be sustained by impulses of the periods $2T$, $3T$, &c.; for if the pendulum-bob is at *a* at the times $0, T, 2T, 3T$, &c., it is sufficient that it should receive an impulse in the direction *ab* at the instants $T, 3T, 5T$ in order to replace the energy lost by friction. It is seen, however, that the amplitude of the pendulum-oscillations will be greatest for equality of periods, if the impulses are in all cases of equal intensity.

From these considerations we should expect that, in the constrained vibrations of the thread, the *m*th partial vibration whose period $T_m = \frac{1}{m} T_1$ is equal to the *m*th part of the period of the fundamental vibration, would appear if the period (*T*) of the excitation is equal to $T_m, 2T_m, 3T_m$, &c. The phenomena of oscillation, however, are not so complicated with the apparatus here described as might appear from this. A small wheel is fixed somewhat excentrically upon the axis of the siren (fig. 2) (the excentricity amounts to about 0.5 millim.), and to the uprights carrying the wheelwork is fixed, by means of two screws, a support for a little bent lever, the conically-pointed axis of which rests in two steel screws passing through the support, and moves in it with a small amount of friction. The wheel itself and the supports for the lever are of cast brass. The lever (as shown in fig. 2) is capable of motion in a vertical plane, presses with its vertical arm against the excentric wheel, and has its horizontal arm attached to the stretched thread, which is horizontal and at right angles to the axis of the lever. The tension of the thread retains the lever in its position when the siren is at rest, and brings it back into its position of rest when the siren is sounding, and the revolving excentric wheel imparts to it periodically a small motion. It is not necessary, nor possible with a considerable tension of the thread, that the lever should touch the wheel

Fig. 2.



in all positions; it is sufficient if the wheel comes into contact with the lever for a short interval of time. The degree of contact may also be easily regulated. At the proper distance from the siren, and opposite to it, a wooden stand is placed, carrying a pulley-wheel turning very easily, such as the wheel of an Atwood's machine. The wheel is adjusted at such a height that the thread which passes over it may be horizontal when stretched by means of weights placed in a little scale-pan attached to it. If the friction between the wheel and the lever is too great in the horizontal position of the thread, the support carrying the pulley-wheel may be raised a little, so that the horizontal arm of the lever may be pulled up a little and the vertical arm thus removed somewhat from the wheel. If the lever does not touch the wheel at all, the support must be lowered somewhat.

When the friction has been properly regulated, the siren may be put into action and its note gradually raised. At first the motion of the thread may be followed through its separate phases with the eye, but when the velocity of rotation of the siren becomes greater there is to be observed only an uncertain trembling of the thread, and the friction of the lever on the wheel produces a noise; then this suddenly ceases, and the thread is seen to be in stationary oscillation of the fundamental form. The amplitude is considerable, and with threads of 1 metre long is often of three fingers' breadth, if the dimensions of the thread have been well chosen. As soon as the thread has settled down to perform its fundamental oscillation with constant amplitude, it is found to be very easy to maintain the velocity of the siren constant. Every one who has used a siren to determine the pitch of a note must have remarked how difficult it is to maintain the note of a definite pitch if there is no arrangement for regulating the pressure of air. I must acknowledge that I had not expected to see this difficulty so easily removed; and it is certainly a peculiar action of the friction between the lever and the wheel, though one easily to be explained*, which causes that a change in the pressure of the air delivered by the bellows produces only a change in the intensity of the note given by the siren, and not at once any change in the velocity of rotation—if this coincides with the velocity of vibration of the thread. The tone of the siren and the vibration of the thread can equally easily be maintained constant, when the velocity of rotation of the siren is increased

* The thread performing its own proper vibration is, as it were, in a condition of rigidity, out of which it is not easily brought (unless its mass is altogether too small), and thus acts, by means of the friction between the lever and the wheel, as a regulator to the siren.

so much that the thread shows its second, third, or fourth mode of vibration ; and the period of excitation becomes equal to $\frac{1}{2}T_1$, $\frac{1}{3}T_1$, $\frac{1}{4}T_1$, where T_1 denotes the period of the fundamental vibration of the thread. According to theory, a definite form of vibration of the thread must make its appearance when the time of rotation of the siren $T=2T_m$, $3T_m$, &c. ; so that, for example, if $T=\frac{3}{4}T_1=3T_4$, the thread vibrates in four half-waves, and for $T=\frac{2}{3}T_1=2T_3$ the thread shows its third form of vibration. The velocity of rotation of the siren can be maintained constant without trouble only when the period of excitation is equal to that of the vibration of the thread ; and if a vibration of the thread belonging to some other ratio of periods shows itself for a moment, it does not produce any perceptible disturbance. It is precisely this peculiarity—that the vibrations of the thread can be maintained without change only when the periods are equal—that renders this form of apparatus so suitable for the demonstration of the laws of the vibrations of threads.

We may now give some examples of the use of the apparatus.

In order to show that the numbers of vibrations of the overtones of a thread are in the ratio of the natural numbers, it is only necessary to count the revolutions of the siren when it puts a thread or a thin metallic wire into vibration with 1, 2, 3, &c. half-waves. In Table I. I have brought together four series of observations, made with four different threads each 1 metre in length. I. denotes a cotton thread with a tension of 50 gr. ; II. bookbinders' thread with a tension of 50 gr. ; III. a thread of button-hole silk with a tension of 9 gr. ; and IV. a brass wire of about 0.15 millim. diameter and 200 gr. tension. The number n denotes the order of the vibration and N the number of vibrations.

TABLE I.

$n.$	N I.	N II.	N III.	N IV.
1.	43.2	33.6	16.7	56.5
2.	86.6	67.1	33.6	112.7
3.	129.1	100.2	50.4	168.2
4.	134.0	67.0

The number of vibrations N was determined from three independent observations—by reading the dials of the siren every 2 minutes, adding together the numbers of revolutions,

and dividing by 360. The numbers of vibrations of the following Tables were determined in the same way.

It is not possible to raise the tone of the siren to any extent, and thus to render visible the higher partial vibrations of the same thread. But for lecture-experiments it will always be sufficient to show the law of overtones for the slower vibrations; and if it is desired to go further than in the experiments I have described, it is only necessary to replace the excentric wheel by an elliptical wheel, or by a toothed wheel exactly centred, with, say, four teeth, so that for the same velocity of rotation the number of shocks is doubled or increased fourfold. I have obtained the best results with toothed wheels of various forms, but think it more convenient so to choose the thread, its length and tension, that the excentric wheel suffices for the experiment.

In order to demonstrate the Law of lengths, it is advisable to take a longer piece of a not too thin white silk thread, which will be nearly the same length for great differences of tension, and to stretch it over a measure and mark off equal distances with India ink. Thus, in the experiments to which Table II. relates, a thread of button-hole silk 2 metres long, and marked off into lengths of 10 centim., was attached to the lever of the apparatus, so that the first mark fell at the point of attachment, passed over the pulley, and stretched with a force of 60 gr. by weighting the scale-pan with shot. Between the siren and the upright carrying the pulley-wheel I placed a table, carrying a small iron support with a clamp to grip the thread. In the Table, l denotes the length of the vibrating thread and N the number of oscillations for the fundamental note.

TABLE II.

l .	N .	$\frac{N_1}{N_n}$.	l .	N .	$\frac{N_1}{N_n}$.
metre			metre		
1.00	54.0	1	1.00	54.0	1
1.00	49.0	1.10	0.90	59.8	0.90
1.20	45.1	1.18	0.80	67.8	0.79
1.30	41.8	1.29	0.70	77.3	0.70
1.40	38.4	1.41	0.60	90.7	0.59

We have therefore $N_1 : N_n = l_n : l_1$; the deviations from the law which the table shows are within the limits of experimental errors.

Thirdly, in order to show that, with threads of the same

material and of the same length, the number of oscillations is proportional to the square root of the tension, a thread of the same silk used in the last experiment and 1 metre long was subjected to various tensions. The tension to begin with was 9 gr., obtained by weighting a small pill-box with shot, and the higher tensions were obtained by adding weights. In Table III., P denotes the load, N the number of oscillations for the fundamental note; the second and third columns show that $\frac{\sqrt{P_n}}{\sqrt{P_1}} = \frac{N_n}{N_1}$.

TABLE III.

P.	$3 \frac{\sqrt{P_n}}{\sqrt{P_1}}$	N.	$3 \frac{N_n}{N_1}$	P.	$3 \frac{\sqrt{P_n}}{\sqrt{P_1}}$	N.	$3 \frac{N_n}{N_1}$
gr. 9	3	17.2	3	gr. 49	7	40.8	7.04
16	4	23.4	4.08	64	8	46.2	8.02
25	5	29.4	5.12	81	9	51.1	8.97
36	6	34.1	5.98				

In order to show that the vibration-numbers of threads of equal length and tension vary inversely as their masses, it is best to employ thin metallic wires, since they are heavier than threads, and consequently a small error in the weights is not so perceptible. The weights, p , of wires employed for the following experiments were determined by weighing four metres of each wire; after being weighed a length of 1.20 metre was cut off and stretched between the siren and the pulley-wheel, so that the length of the vibrating portion was 1 metre; the tension (200 gr.) was chosen so great that the portion of the wire depending from the pulley-wheel did not need to be taken into account.

TABLE IV.

Material.	p .	$\sqrt{\frac{p}{p_n}}$	N.	$\frac{N_n}{N_1}$.
German-silver ...	gr. 0.660	1	90.5	1
Brass	1.604	0.641	57.2	0.63
Iron	2.212	0.545	50.6	0.56

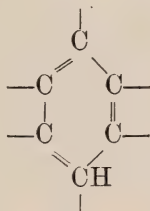
After this description of the demonstration of Mersenne's laws by means of this apparatus, it will not be necessary to

say more of the advantages which it offers to the demonstrator of physics. It is, however, perhaps not superfluous to remark, that the apparatus is so little costly that any institution at which instruction in physics is given may be able to obtain it. A bellows and wind-chest for organ-pipes and a siren belong to the necessary apparatus of instruction in acoustics; we require in addition only a small piece of apparatus which may easily be fitted to any siren. The lever arrangement must, however, be well made, so as to avoid disturbing noises, which might easily be produced by the friction of the lever against the revolving wheel.

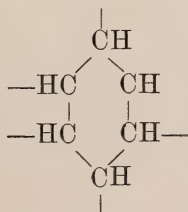
The apparatus for my own experiments was made by the University apparatus-maker, Herr Engel, of Marburg.

VI. *The Influence of Atomic Arrangement on the Physical Properties of Compounds.* By W. N. HARTLEY, F.R.S., Professor of Chemistry, Royal College of Science, Dublin*.

IN the Philosophical Magazine for March 1882, Dr. Carnelley states that Professor Julius Thomsen, of Copenhagen, has concluded from the heat of combustion of benzene that the six carbon-atoms in the compound "are not bound by three double and three single linkings, thus,



but by nine single linkings, thus:—

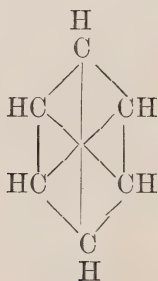


Bruhl, however, as we shall see presently, concludes from the

* Communicated by the Author.

specific refraction of benzene that the old view is the correct one." I presume that Dr. Carnelley was here referring to the paper in the *Berlin Berichte*, vol. xiii. p. 1808 (1880); and if this is so, there is little doubt that he has misinterpreted Thomsen's conclusions, as the formula given represents only six single linkings. The "heat of combustion of benzene, if the carbons are linked as in Kekulé's formula, would amount to 846,000 units, whereas if there are nine single linkings in the molecule it would be 802,330 units. The experimental number obtained by Thomsen was found to be 805,800; hence he concludes that the six carbon-atoms in benzene are nine times singly linked with each other." I quote here from my own paper, entitled "Researches on the Relation between the Molecular Structure of Carbon Compounds and their Absorption-Spectra," published in the *Journal of the Chemical Society* for April 1881; and my conclusion was that, "subject to the correctness of this deduction, I consider the following conclusion to be established:—*No molecular arrangement of carbon-atoms causes selective absorption unless each carbon is itself united to other three-carbon atoms, as in the case of benzene.*"

In accordance with this view, benzene could be represented by a formula such as the following:—



which is practically the same as Ladenburg's prism; and we have yet to learn that this is not in harmony with Bruhl's determination of the refraction equivalent. It may be observed that I have carefully guarded myself against unconditionally accepting this last expression, since there are other considerations which tend towards favouring that of Kekulé. Single, double, or treble linkings are simply an incomplete method of representing the relation of the carbon-atoms to each other at some particular phase of their vibrations. Such representations are fictitious, since they give us no idea of the vibrations which are the cause of all the properties of these

compounds. I therefore regard that expression as the best which commits one in the least degree to any very decided view of the inner construction of benzene, and such a representation is the simple hexagon.

VII. *Permanent Magnets*.—II. *On Magnetic Decay; with a Correction to the Value of H at Oxford*. By R. H. M. BOSANQUET, *St. John's College, Oxford*.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE decay of the magnetism of permanent magnets is a subject of the greatest practical importance. Some of the best makers of electrical measuring instruments continue to rely to a considerable extent on permanent magnets, and the change of these is not easily eliminated with accuracy.

Among the methods employed for the discovery of the changes in permanent magnets, the direct determination of their moments, which accompanies the ordinary process for the determination of the horizontal component of the earth's magnetism, seems to be free from objection. This is the method some further results of which I propose now to communicate.

The existence of the decay or diminution of magnetic moments has been pointed out by Joule*, and is alluded to in my former paper (*Phil. Mag.* xvii. p. 438). The values now dealt with form a sequel to those given in that paper.

It is important to consider the justification offered for the continued use of magnets as standards, and this depends on the process used for the redetermination of the constants involved.

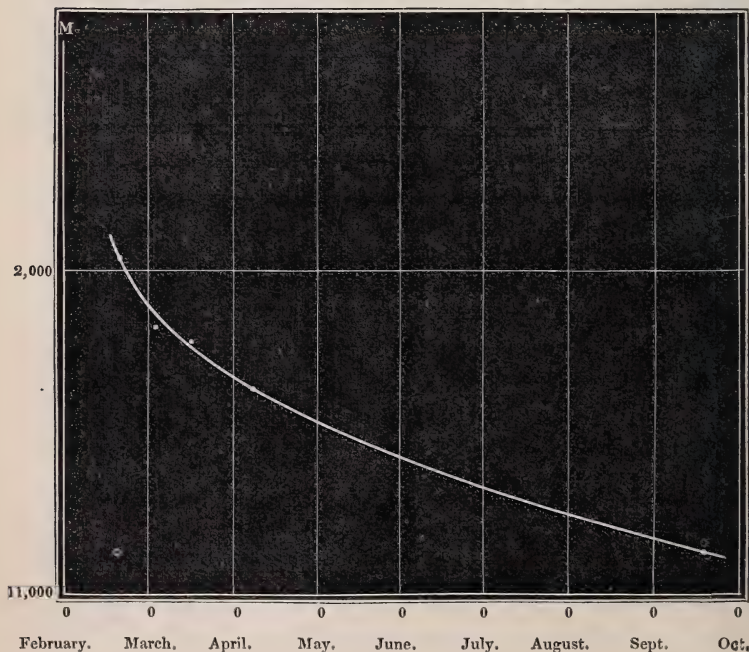
One of the processes consists ultimately in the reference to a Clark's cell, which is supposed to remain perfectly constant. The assumption is justified by the fact that two such cells similarly prepared at the same time do not vary relatively to each other, though their values are not generally identical. It appears, however, that two such similar cells, being exposed always to the same influences, might be expected to make any changes they do make in common. At all events, without some further check, there is nothing in the shape of absolute proof attainable by this method as to the constancy of the ultimate standard.

* Reprint, i. p. 591.

The temporary effects of changes of temperature on permanent magnets are also well known to be such as would certainly vitiate any accurate measurements founded on such magnets, in the absence of special precautions.

The following Table contains the mean results of sets of observations made at different times during the present year. The first two entries contain the observations made use of in the paper above referred to, but divided into two groups and corrected for an improved value of the moments of inertia of the magnets.

Mean date.	Number of sets.	M.	H.	Place.
February 18 ...	9	12039	·18044	Nonmagnetic room.
March 3	6	11822	·18075	„ „
„ 15	12	11767	·17261	Laboratory.
April 8	11	11620	·17224	„
September 18...	16	11119	·18020	Nonmagnetic room.
„ „	14	11121	·17370	Laboratory.



The curve in the figure exhibits the course of the values of M . In seven months it has diminished very nearly in the proportion of 12 to 11.

These magnets are of what is called best cast steel, and are as hard as they possibly can be made. They were prepared about February 8th.

It is incumbent on those who maintain that permanent magnets can be made subject only to insignificant changes, to submit their magnets to some long-continued series of tests of equal conclusiveness with the above. I hope to continue the series.

The new value of the moment of inertia of the magnets was obtained by a series of forty experiments, in which the magnets were vibrated alternately alone and with the standard brass bar which was made for the purpose. The wire used for suspension was better than that employed on the former occasion. The torsion due to it was considerably less than that of the former wire, and was very nearly the same for all weights.

The values of the moment of inertia arrived at were,

$$I. = 13472, \quad II. = 13369;$$

the old values were

$$I. = 13370, \quad II. = 13334,$$

which were observed upon at the time as unsatisfactory. The correction due to this change is obtained by adding to the log.'s of both M and H the number $\cdot 00111$. Applying this correction to the ultimate value of H formerly obtained, we have:—

Old value of H	$\cdot 18011$
Corrected value	$\cdot 18056$

for the beginning of March 1884.

The mean of the values for March and September is

$$\cdot 18038.$$

The mean Greenwich value for the year, according to the formula in Everett's 'Book of Units,' is

$$\cdot 18186.$$

VIII. *Notices respecting New Books.*

Geology of Wisconsin. Survey of 1873-9. Vol. I. Part I. General Geology. II. Natural History. III. Industrial Resources. Published under the direction of the chief Geologist, by the Commissioners of Public Printing, in accordance with Legislative Enactment. 8vo, pages i-xxiv, and 1-725. With 11 plates, and 157 other figures. [Madison?] 1883.

IN agreement with the plan laid down at first, the general introductory volume is here published last*, although termed "vol. i." The "General Geology" is treated under several heads, so that any one, taking up and desiring to understand the results of the Geological Survey, may make himself acquainted with the principles of the science by which the Survey is carried out. Thus the Chief Geologist, Prof. T. C. Chamberlin, has been called on to supply, and does supply very satisfactorily, a good series of observations and teachings on—I. Chemical Geology, in relation to the Earth's Crust; II. Lithological Geology, as to rocks in groups and individually,—offering a system of nomenclature, and defining technical terms; III. Historical Geology, under the subordinate headings of Laurentian, Huronian, Keweenawan, Cambrian and Potsdam, Lower-Silurian or Cambro-Silurian, Upper-Silurian, Devonian, Carboniferous, Reptilian, Tertiary, and Quaternary periods, and their stratal representatives; with many interesting remarks on the processes, results, and concomitants, occurring from time to time in the geographical and biological conditions of the Earth's surface.

Part II. contains chemical analyses of minerals, rocks, ores, and waters of Wisconsin, some compiled by R. D. Salisbury (pp. 303-308); some made, in a classified system, by R. D. Irving (pp. 309-339); and the lithology of Wisconsin by the same (pp. 340-361). R. P. Whitfield gives a valuable classified list of the Fossils of Wisconsin (pp. 362-375); G. D. Swezey catalogues the Phænogamous and Vascular-Cryptogamous plants of Wisconsin (pp. 376-395). A partial list of the Fungi of Wisconsin, with descriptions of new species, is contributed by W. F. Bundy (pp. 396-401), who gives also an account of the Crustacean Fauna of Wisconsin, with descriptions of little-known species of *Cambarus* (pp. 402-405). Other interesting natural-history communications are—the Catalogue of the Wisconsin Lepidoptera (pp. 406-421), and of the Cold-blooded Vertebrates of Wisconsin (pp. 422-435), both by P. R. Hoy. The late Moses Strong's list of the Mammals of Wisconsin follows (pp. 437-440).

Of the "Economic relations of Wisconsin Birds" an elaborate, careful, and useful (at all events suggestively useful) memoir, by F. H. King, is given (pp. 472-610). The results of the examination of stomachs and food are shown by notes and tables very fully, as to the cases in which Birds are *beneficial*,—destroying "noxious

* See Phil. Mag. for April 1880, p. 302.

plants,' "injurious mammals," "detrimental birds," "injurious reptiles," "noxious insects," "injurious molluscs," and other noxious forms of life, and when feeding on carrion. So also of *harmful Birds*, which destroy useful creatures, their young or their eggs, or when eating the parasites hurtful to obnoxious animals. The relations of Birds to different industries, also their habits, seasons, value as food, æsthetic value, and economic classification, are all considered. Numerous figures of Birds and their various prey are freely inserted in the descriptions.

Part III. treats of Economic Geology. The Iron-ores are described by R. D. Irving (pp. 613-636). The late Moses Strong wrote the Chapter (for the most part) on Lead and Zinc-ores (pp. 637-655). Prof. T. C. Chamberlin has contributed the remaining Chapters—"Economic Suggestions as to Copper, Silver, and other ores" (pp. 656-662); on "Building Material" (pp. 663-677); on the "Soils and Subsoils of Wisconsin" (pp. 678-688); and on "Artesian Wells" (pp. 689-701). A short Note on the Geodetic Survey of the State, and its progress, is given at p. 702, and plate xi. is a map in illustration.

The good Index (pp. 703-725) is well worthy of this excellently useful volume.

Containing much that is of interest to Botanist, Ornithologist, and other Naturalists, this volume is also very useful to the Geologist and Mineralogist. By the Geologist the many figures of fossils, and the highly suggestive hypothetical maps of the North-American land in the several periods of the Huronian, Potsdam, Trenton, Niagara, and Hamilton formations—and again during the First and Second Glacial Epochs—will be fully appreciated. We cordially thank the Scientific Staff and the State Authorities of Wisconsin for this and the other volumes of their carefully-worked Survey of the State.

The Student's Handbook of Physical Geology. By A. J. JUKES-BROWNE, B.A., F.G.S. 8vo. Pp. xii, 514. London: George Bell and Sons. 1884.

ELEVEN years have now elapsed since the second edition of Jukes's 'School Manual of Geology' was prepared by his nephew, the author of the present work. Now the School (or Student's) Manual has, we are informed, been allowed to fall out of circulation, and the 'Student's Handbook of Physical Geology' in a measure takes its place, dealing with a portion of geological science under the headings of Dynamical, Structural, and Physiographical Geology. The Palæontological and Historical sections are left to form another volume.

The work before us, although differing from it in arrangement, is a capital introduction to the more elaborate treatise by Prof. A. H. Green, a new edition of which was issued in 1882. We trust that Mr. Jukes-Browne may complete the second portion of his work more speedily than has been the case with Prof. Green; and

if it prove, which we do not doubt it will prove, as clear and systematic as the 'Physical Geology,' it will furnish a welcome guide to those who may be dismayed at the size and cost of such a work as that produced by Dr. A. Geikie. If, however, one work leads up to the other, the success will be satisfactory, both to student and author.

We might naturally expect to find Mr. Jukes-Browne more "at home" when he comes to deal with the succession of stratified rocks and their fossils, a subject reserved for his second volume; for he is perhaps best known to geologists through his successful labours among the Cretaceous and Post-Tertiary deposits. But a perusal of the present work enables us to speak in the highest terms of the wide research and full treatment of the subjects he has now brought before us. Illustrations are drawn from all parts of the world, and the author evidently gathers inspiration, as well as many duly acknowledged explanations, from the works of Lyell, Jukes, A. Geikie, O. Fisher, and other past and present leaders in Geology.

Here and there we are disposed to differ in the use of terms or on theoretical questions. For instance, on p. 67 the author speaks of "the detrition of a district," and "the denudation of fresh rock-surfaces," when we should transpose the terms. Mr. Jukes-Browne strongly advocates the hypothesis that our Boulder-clay was chiefly formed by Coast-ice, or, in other words, that it was a marine deposit formed near ice-clad shores, but always under water. It has, however, not been shown that Coast-ice is capable of forming a comparatively uniform deposit of Boulder-clay spread over a wide area, usually devoid of any appearance of stratification, and containing no marine shells that lived on the spots where their remains are imbedded. It is true that the advocates of the formation of Boulder-clay by land-ice have to be equally guided by inference; but the frequent contortions and disturbances in beds underlying Boulder-clay, evidently produced by the agent which formed this glacial drift, seem to indicate a force more potent and extensive than coast-ice.

Mr. Jukes-Browne has, however, taken pains to state the opinions opposed to those he may express or adopt; and we doubt not it will be some time before there is unanimity amongst geologists on the subject of the formation of Boulder-clay.

We should not omit to mention that Prof. T. G. Bonney contributes to this work a short chapter on "Original or Igneous Rocks." The subject of rocks or of minerals cannot be made interesting, in the ordinary sense, to the student; nor for that matter can we say that the present volume is one to be read through lightly and easily by a general reader. But it is full of facts and information, and as such is just the work to be appreciated by the young and earnest student, who desires to learn about volcanoes and earthquakes, springs, the formation of various deposits, the structure of rocks, and the origin of hill and valley, mountain, lake, and river.

IX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xviii. p. 386.]

November 5, 1884.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S.,
President, in the Chair.

THE following communications were read:—

1. On a new Deposit of Pliocene Age at St. Erth, 15 miles east of the Land's End, Cornwall." By S. V. Wood, Esq., F.G.S.

2. "The Cretaceous Beds at Black Ven, near Lyme Regis, with some supplementary remarks on the Blackdown Beds." By the Rev. W. Downes, B.A., F.G.S.

3. "On some Recent Discoveries in the Submerged Forest of Torbay." By D. Pidgeon, Esq., F.G.S.

November 19.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "Note on the Resemblance of the Upper Molar Teeth of an Eocene Mammal (*Neoplagiaulax*, Lemoine) to those of *Tritylodon*." By Sir Richard Owen, K.C.B., F.R.S., F.G.S.

2. "On the Discovery in one of the Bone-caves of Creswell Crag of a portion of the Upper Jaw of *Elephas primigenius*, containing, *in situ*, the first and second Milk-molars (right side)." By A. T. Metcalfe, Esq., F.G.S.

3. "Notes on the Remains of *Elephas primigenius* from the Creswell Bone-cave." By Sir R. Owen, K.C.B., F.R.S., F.G.S., &c.

4. "On the Stratigraphical Position of the Lower and Middle Jurassic *Trigonia* of North Oxfordshire and adjacent districts." By Edwin A. Walford, Esq., F.G.S.

December 3.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "Note on a Section near Llanberis." By Professor A. H. Green, F.G.S.

In this paper the author described a section showing actual unconformity at the base of the Cambrian. In one of the cuttings on the railway that runs from the Dinorwig quarries along the north-western shore of Llyn Padarn, the basement conglomerate of the Harlech and Llanberis group, dipping N.W. at a moderate angle, rests upon vertical flaky beds, one of which is a breccia.

The author believes that the section described is one of which a different reading was given by Sir A. Ramsay in the Geological-Survey Memoir on North Wales. In that work the conglomerate was regarded as a continuation of the breccia of the underlying beds sharply turned over.

The microscopic characters of the lower series show that these beds are probably coarse volcanic tuffs, and that they resemble the rocks at St. David's, called Pebidian by Dr. Hicks. The unconformity observed does not necessarily indicate great difference of age between the conglomerate and the underlying beds. In volcanic rocks such breaks may be merely local.

Further north-west, in the same railway section, a junction is seen between the Cambrian conglomerate and quartz-felsite. It is uncertain whether the junction is a fault or not. The matrix of the conglomerate is chiefly composed of felsite fragments so perfectly cemented together as to bear a close superficial resemblance to the original rock even under the microscope, unless a high power be used.

2. "The Tertiary Basaltic Formation in Iceland." By J. Starkie Gardner, Esq., F.L.S., F.G.S.

The country explored stretches from the N.E. corner to the S.W. Every locality in which lignite had been met with was visited. The most northerly of these, at Húsavík, presents a coast-section showing 200 feet of tuffs with bands of lignite, 200 feet of the same with marine shells, and an immense series of overlying tuffs, which are unfossiliferous, and were followed, ten miles further north, to Tjórnes, almost within the Arctic Circle. The shells, a series of which were exhibited, indicate a warmer sea, and, in the author's opinion, are of an age a little anterior to the Crag. It is hoped that Dr. Gwyn Jeffreys, who has several times examined them, may pronounce a definite opinion in regard to this. A number of sections towards the interior were visited, one of the finest being in a cañon near Hof, where the sides are upwards of 1000 feet high, and nearly vertical, exhibiting an alternation of semicolumnar basalts, ash-beds, and laterites, capped by rhyolites. These rhyolites are very beautiful, and cap the basalts over a wide area, being themselves overlain by other and more irregular streams of basalt and tuffs. The country has been subjected to immense denudation, and is cut up into rolling flat-topped hills such as characterize basaltic regions elsewhere. The horizon from which most, if not all, the fossil plants from Iceland have been obtained, is that of the rhyolites—a more recent series than any represented in the British Isles or even in the Faröes. Their age may have been correctly assigned to the Miocene.

3. "On the Lower Eocene Plant-beds of the Basaltic Formation of Ulster." By J. Starkie Gardner, Esq., F.G.S.

X. *Intelligence and Miscellaneous Articles.*

ON THE EMPLOYMENT OF MARSH-GAS FOR PRODUCING EXCEEDINGLY LOW TEMPERATURE. BY M. CAILLETET.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE just received the September Number of the Philosophical Magazine, in which I read with surprise a communication by Professor J. Dewar claiming the priority for employing marsh-gas in order to obtain very low temperature. Mr. J. Dewar, to prove his claim, alludes to a note published by him in 'Nature' of the 4th of October, 1883, which, he says, "will prove that my experiments with liquid marsh-gas were made a year in advance of those made recently by M. Cailletet."

It is to be regretted that Mr. J. Dewar, who must be well acquainted with the contents of the communications presented by me to the French Academy of Sciences on the use of Marsh-gas, as well as the date of its presentation, has not spoken of the Notice I was obliged to publish a few weeks afterwards to refute the unjust claim of priority alleged by M. Wroblewski. This Note was deposited by me, closed and sealed, in the hands of the President of the French Academy on the 12th of December, 1881, that is to say, *two years previous to the publication made by Mr. J. Dewar in 'Nature.'* The Note was opened at my request and read in the Academical Meeting of the 4th of last August. It contains these words:—"I am busy working up researches, at this moment, which will take me a long time, and will not be published before a lengthened period. I have been obliged to speak of the details of these researches to several persons; it is therefore quite necessary for me to establish early my rights to priority in case any author were to take precedence, and for that object I write this note, which contains the summary of my work. I am trying to obtain greater cold than that obtained until now by scientific men. . . ."

"By means of a pump, the piston-rod of which is covered with mercury, preventing thereby air and other injurious impediments, I can obtain the liquefaction of great quantities of carbonic acid and protoxide of nitrogen, as also of *marsh-gas* and ethylene, whose critical points are greatly inferior to those of carbonic acid. I am nearly certain to obtain very low temperature by plunging my instruments in these liquefied gases. I hope, lastly, to be able to liquefy oxygen, nitrogen, and other gases completely by refrigerating them by means of ethylene or marsh-gas boiling, after having compressed them in a curved tube by employing the instrument invented by me and which is well known to the Academy."

This Note cannot leave, I trust, any doubt of the priority of my researches over those of Mr. J. Dewar; and it is certainly owing to his not having known it that he has published in the Philosophical Magazine the article which I feel entitled to answer.

L. CAILLETET,

Paris, 10 décembre, 1884.

Membre de l'Académie des Sciences.

Phil. Mag. S. 5. Vol. 19. No. 116. *Jan.* 1885.

F

ON THE VALUE OF POISSON'S COEFFICIENT FOR CAOUTCHOUC.

BY E. H. AMAGAT.

It is known that scientific men are far from being in agreement as to the numerical value to be given to what is known as *Poisson's Coefficient*. According to the theories of Poisson, Navier, M. de Saint Venant, and the experiments of Cagniard de Latour, and of M. Cornu, this coefficient should be equal to $\frac{1}{4}$; according to Wertheim it is equal to $\frac{1}{3}$; from the researches of Cauchy, of Lamé, and of Kirchhoff, all that can be affirmed is that it is between zero and $\frac{1}{2}$; finally, according to MM. Schneebeli and Okatow it varies not only from one body to another, but also with the same body according to its physical condition. The latter physicist had the idea of utilizing the great extensibility of caoutchouc to determine the coefficient in question by the well-known method; and MM. Naccari and Bellati have made analogous experiments by Regnault's method.

It has been observed with justice that experiments made with caoutchouc are but little conclusive; they present great irregularities, due more especially to permanent deformations; and, on the other hand, the body has but little homogeneity. It may be added that the very magnitude of the deformations has put it outside the theoretical conditions, which suppose the deformations to be very small.

I propose to show that by following a totally different path we may arrive, in the case of caoutchouc, at conclusions which can only be invalidated by supposing errors of experiment quite out of proportion to those which can be really committed.

In order to make these experiments, and others which are not concluded, I have had a piezometer made, in which, as in that of Regnault, the pressure may be transmitted in the interior or on the exterior at the same time or separately. The apparatus is, however, double; two spheres or two cylinders may be placed in it side by side, which are under precisely the same pressure and at the same temperature—conditions which are very favourable for rather delicate comparative experiments. This is not the case of the present experiments, which, as we shall see, do not claim great accuracy.

I call σ Poisson's coefficient, K the coefficient of cubical compressibility, α the coefficient of elongation or the inverse of the coefficient of elasticity, and λ and μ the two constants.

We have the ratios :—

$$(1) \quad \sigma = \frac{\lambda}{\alpha(\lambda + \mu)},$$

$$(2) \quad K = 3\alpha(1 - 2\sigma),$$

$$(3) \quad \frac{1}{\alpha} = \mu \frac{3\lambda - 2\mu}{\lambda + \mu}.$$

For two different bodies,

$$(4) \quad \frac{K}{K'} = \frac{\alpha(1 - 2\sigma)}{\alpha'(1 - 2\sigma')}.$$

I place in the different apparatus two spheres, one of caoutchouc and the other of bronze, which I assume are exactly identical, so as to simplify the reasoning; a few millimetres of pressure are sufficient to raise the water 300 divisions in the rod of the caoutchouc sphere; in the other the motion of the meniscus is barely perceptible, and it cannot be measured with accuracy. It follows from this that (referring to the formula, which gives in this case the variation of the internal volume), whatever may be in the case of bronze the value of σ between zero and $\frac{1}{2}$ (the accented letters refer to bronze), α is very great in reference to α' .

It might be feared that in this case a considerable amount of the diminution of volume arises from a change of form; but this first operation might be replaced by a direct determination of α by means of traction; we obtain the same result. In one of my experiments $\frac{\alpha}{\alpha'}$, was equal to 60,000 in round numbers.

That being so, let us compress internally and externally at the same time: according to the formula relative to this case, the changes of internal volume will be proportional to K and K' ; K should therefore be very great in comparison with K' , the compressibility would even be relatively negligible, and the liquid should ascend in the stem of the caoutchouc sphere; but nothing of this kind takes place: the water sinks whatever be the pressure, and gives the inevitable irregularities with caoutchouc. It would be difficult to say whether the variation of volume has been greater with this body or for bronze, so small is the mean difference; hence α is comparable to α' , and is perhaps even smaller. It evidently follows from this that the ratio (4) can only be satisfied provided that $\frac{1-2\sigma}{1-2\sigma'}$ is very small, and therefore σ very little different from $\frac{1}{2}$.

Assuming for σ' the number $\frac{1}{3}$, we have from the preceding

$$\sigma = \frac{1}{2} \left(1 - \frac{K}{K'} \times \frac{1}{180000} \right).$$

Whatever errors may be attributed to want of homogeneity, to small deformations, and to permanent deformations, even if the results found were double, or even tenfold, it may be said that σ would still be very little different from $\frac{1}{2}$, and far higher for example than 0.499.

We may, moreover, by means of general formulæ and without comparing caoutchouc to another body, arrive at the same conclusion in different ways, which essentially amount to this: the coefficient of cubical compressibility is very small, as actual experiment shows; as moreover it is equal to $3\alpha(1-2\sigma)$, and since α is very great, $1-2\sigma$ must be very small, and therefore σ very near $\frac{1}{2}$.

There is nothing contradictory in this result. It follows from that and from the ratio (1) that μ is very small; and therefore from ratio (3) that α is very great: this is in fact the case.

But here a great difficulty presents itself. The various caout-

choucs which I have examined have all led to the same result; it would certainly be the same with that which Wertheim used, and this physicist found, by measuring directly the diminution of cross section, $\sigma = \frac{1}{3}$. Since then MM. Naccari and Bellati have shown, by Regnault's method, that for the same substance the value of σ may amount to 0.41; but this number satisfies the ratio (4) no better than that of Wertheim; assuming $\sigma' = \frac{1}{3}$ it would give $\frac{1-2\sigma}{1-2\sigma'} = 0.36$, a value quite out of proportion to that which should be found.

As it is difficult to assume that Wertheim, and then MM. Naccari and Bellati, have made experimental errors as great as appears from the preceding,—as, on the other hand, there is no reason why caoutchouc supposed to be homogeneous, and submitted to small deformations, should not conform to general laws, it may be asked how far the formulæ assumed are really the expression of these laws. It is not, then, superfluous to submit these formulæ to the test of experiment, following in this the advice given by Regnault; it is with this object that I have had made the differential apparatus mentioned above; these researches are in course of execution.—*Comptes Rendus*, July 21, 1884.

ELEMENTARY PHYLLOTAXY. BY PLINY EARLE CHASE, LL.D.*

I have shown† that the “numerics” of the chemical elements can be better represented by various phyllotactic divisors than by Prout's law. Phyllotactic submultiples of the organic elements C, H, O, N give a mean residual ratio of .05654, while Prout's law gives .12007, the probable ratio of merely accidental residual being .18394. This indicates comparative aggregate probabilities which are represented by the reciprocals of .05654⁶⁵, .12007⁶⁵, and .18394⁶⁵, or by 1999×10^{30} , 1098×10^9 , and 1.

In the Philosophical Magazine for November 1884, Dr. Edmund J. Mills gives additional evidences of phyllotactic influence. He represents all the elementary numerics, except that of hydrogen, by the equation

$$y = p15 - 15(.9375)^x.$$

This introduces the first five numbers of the phyllotactic series 1, 2, 3, 5, 8, being of the form

$$5 \times 3[p - (\overline{5 \times 3 \div 2 \times 8})^x].$$

The sum of the infinite series which is represented by $15 \times (\overline{5 \times 3 \div 2 \times 8})^x$ is the product of the first five phyllotactic numbers, $1 \times 2 \times 3 \times 5 \times 8$. The series itself is of the form $(n \div \overline{n+1})^x$, thus representing cumulative harmonic rupturing tendencies, of the same kind as are shown in the inter-stellar influence upon planetary positions (*Phil. Mag.* September 1884, p. 197).

* Communicated by the Author.

† *Proc. Amer. Phil. Soc.* xix. pp. 591–601; xx. p. 431, &c.

ON A NEW FORM OF POLARIZING PRISM. BY C. D. AHRENS.

The prism which I desire to bring to the notice of the Society is intended for use either as a polarizer or an analyzer. It will, I hope, be found especially useful as an analyzer for the microscope.

The employment of a Nicol prism above the eye-lens is subject to the great inconvenience that, owing to the necessary length of the prism, the eye of an observer is so far removed from the lens that a portion of the field is cut off. Double-image prisms of the usual construction are shorter, but they have another defect, viz. that the angular separation of the rays is so slight that the eye sees both images at once, and some confusion is thus caused.

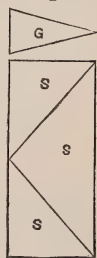
My object in constructing this improved prism has been to obtain a much wider separation of the two beams of light; so that one of them, although not actually removed entirely by total reflection (as in the Nicol prism), is so far refracted to one side that it may be neglected altogether. I made several attempts to construct such a prism some years ago, but failed (as probably others have done) owing to the difficulty or impossibility of avoiding distortion and colour, and of obtaining a wide separation of the ordinary and extraordinary rays in a prism made up of only two pieces of Iceland spar.

I have now effected the desired object by making the prism of three wedges of spar cemented together by Canada balsam, as shown in the accompanying drawing (fig. 1). The optic axis in the two outer wedges is parallel to the refracting edge, while in the middle wedge it is perpendicular to the refracting edge, and lies in a plane bisecting the refracting angle. This disposition of the optic axis is the one originally suggested by Dr. Wollaston, and has the effect of causing a greater angular separation of the rays than Rochon's construction. By the employment of three prisms instead of two I am able to give the middle prism a very large angle, and yet to correct the deviation of the rays so far that on emergence they make approximately equal angles with the central line of the combination.

Nearly in contact with one of the terminal faces of the prism I place a prism of dense glass of such an angle that it just corrects the deviation of one of the rays and also achromatizes it, while it increases the deviation of the other ray to such an extent that it may be practically disregarded altogether; an eye, even when placed almost close to the prism, receiving only the direct beam. This beam is, of course, perfectly polarized in one plane, and can, by a proper arrangement of the glass compensator, be rendered practically free from distortion and colour.

Other methods of effecting the compensation have suggested themselves in the course of my work; and I have obtained the best results by adopting the arrangement represented in fig. 2.

Fig. 1.



In this, the glass compensating prism, instead of being mounted separately, is cemented upon one of the terminal faces of the compound spar-prism; the angle of this latter, and also of the other terminal face, being suitably modified.

This seems distinctly preferable to the original arrangement, for several reasons.

1. The total length of the compound prism is rather less, being scarcely more than twice its breadth.

2. The field is rather larger, so that the prism can be used over microscope eye-pieces (A and B) without any of the field of view being cut off.

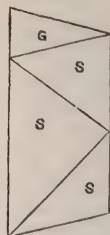
3. The whole arrangement is more compact, all the components being firmly cemented together, and therefore not liable to accidental displacement.

4. There is less loss of light by reflection, the reflecting surfaces being reduced to two.

A ray of light entering the prism in a direction parallel to its axis is divided into two rays; one of which, on emergence, follows a course parallel to that of the original incident ray, and is practically free from distortion and colour; the other ray is deviated to the extent of about $59^{\circ} 30'$ (for yellow sodium-light), being, of course, strongly coloured and distorted. The angular separation is so great that this latter ray does not interfere with ordinary observations.

I hope that the prism, which has cost me much time and labour, will meet with the approval of the Society, and take a place as a useful accessory to the microscope and other optical instruments.—*Journal of the Royal Microscopical Society*, August 1884.

Fig. 2.



ON THE PENETRATION OF DAYLIGHT IN THE WATER OF THE LAKE OF GENEVA. BY MM. FOL AND ED. SARASIN.

Questions relating to the absorption of light by more or less thick layers of the very pure water of the Lake of Geneva have been the object of a series of experiments, undertaken by a Commission of the Société de Physique et d'Histoire naturelle of Geneva, on the incentive, and under the direction, of M. Louis Soret*.

We have been charged more particularly to examine, by means of photography, the extreme depth which daylight reaches. Our experiments consisted in exposing a photographic plate at various depths in that part of the lake where the water is deepest.

We used Monckhoven's rapid gelatino-bromide plates. They were placed in a special apparatus designed by one of us for these experiments†. It consists of a brass photographic back, the two

* *Comptes Rendus*, March 10, 1884, p. 624, and *Archives des Sciences physiques et naturelles*, vol. xi. p. 327. vol. xii. p. 158, 1884.

† This apparatus was constructed according to the designs and instructions of M. Fol by the Société genevoise d'instruments de Physique.

slides of which are closed by the action of a pair of levers joined like scissors and drawn by a weight ; they separate by the action of an antagonistic spring, as soon as the weight of the lead in touching the bottom ceases to act on the levers. Knowing the depth, we can regulate the length of the line by which the weight is suspended to the apparatus, so as to have the photographic plate exposed in a horizontal position, at a given distance below the surface of the water. After a given exposure the apparatus is withdrawn, and rapidly closes under the action of the weight. The time of exposure was ten minutes in all cases. The development was made with the normal oxalate-of-iron developer, which was caused to act uniformly for ten minutes on each plate. They were all of the same lot, and therefore coated with the same emulsion*.

The experiments were made in front of Evian, where the lake has over a large surface a depth of 315 metres. Dr. Marcet was kind enough to place at our disposal on two occasions his steam-yacht, the 'Heron.' Professor Forel, of Morges, had the goodness not only to lend us his sounding-line, but also to accompany us, and help us with his advice and his experience.

On the 16th of August, in calm weather and with a brilliant sun, we exposed:—

1. At 237 metres deep two plates, one at half-past twelve and the other at seven minutes past one.
2. At 113 metres a plate at twenty minutes after two.
3. At 300 metres deep (15 metres from the bottom) a plate at forty-four minutes past two.

On the 23rd of September, 1884, in cloudy, but fine weather, the clouds slight and rather luminous, light wind varying from east to north, we exposed:—

4. At 147 metres a plate at 1 o'clock.
5. " 170 " " 26 minutes past 2.
6. " 113 " " 3 " 3.
7. " 90.5 " " 34 " 3.

As a comparison, we exposed on the 15th of August at 10 p.m., on a clear night, but without moon:—

8. A plate in the open air for ten minutes.
9. " " " five "

On developing, it was found that the plate 3 (300 metres deep) had received no luminous impression whatever. The same was the case with plate 1. Plate 5 at 170 metres was slightly clouded, almost like plate 9, which was exposed at night for 5 minutes. Plate 4 at 147 metres had been slightly acted on, more so than the plate which had been exposed at night for ten minutes. Of the two plates at 113 metres, the plate 6 of the

* Preliminary experiments in rather shallow water showed that the apparatus worked as well as could be desired, and that when closed it could be left in full sunlight without any light getting to the plate. During exposure the glass was uppermost and towards the light. On this side characters and numbers were traced in black varnish. The sharpness with which these signs were produced in white on the developed plate, the purity of the edges of the image, which, restricted by the separation of the covering plates, only extended to the middle of the sensitive layer, prove that light could not penetrate accidentally.

second day was strongly blackened, while plate 2 of the first day was no more affected than plate 4 of the second day. Finally, plate 7, exposed at 90 metres, was so acted upon that the lines which had been traced on the back were only imperfectly seen on the dark ground of the developed layer.

Comparing the results obtained on the two days of the experiments, we are struck by the fact that the photographic action was greater on the 23rd September than on the 16th August.

We are thus led to conclude from these first attempts ;

1. That daylight penetrates into the water of the Lake of Geneva to a depth of 170 metres, and probably further ; that at this depth the illumination in full daylight is just comparable to that which we observe on a fine night when there is no moon.

2. That at 120 metres the light is still very strong.

3. That in September, in cloudy weather the light penetrates in larger quantity and to a greater depth than in August, in perfectly bright weather. Further experiments will have to decide whether this difference is to be attributed to the greater transparency of water in autumn and in winter, which the experiments of M. Forel* have put beyond doubt, or whether the light diffused from the clouds penetrates further than the more or less oblique rays of the sun.

Previous to our experiments, M. Asper† had exposed gelatino-bromide plates in the Lake of Zurich at depths varying from 40 to 90 metres. He immersed them at night, and withdrew them the following night. But the darkest night is still bright for a rapid gelatino-bromide plate. Our experiments seem then to be made under more trying circumstances. We intend to pursue these experiments in the summer of 1885.

We are desirous also, if possible, of making analogous experiments in the sea, where the greater transparency of the water leads to the supposition that the extreme limit of the luminous rays is at a still lower level.

In this respect satisfactory data are wanting, for the experiments of the cruise of the 'Porcupine' have remained in the state of project, as the apparatus devised by Sir W. Siemens refused to act. The depth to which daylight penetrates in the sea is therefore still to be found.—*Comptes Rendus*, Nov. 10, 1884.

* M. Forel investigated from 1873 the transparency of the waters of the Lake of Geneva by a photographic method ; he employed, however, albuminized silver paper, which is less sensitive than gelatino-bromide. He immersed the sheets at night at various depths, and took them out after one or several twenty-four hours. This method gave for the limit of absolute darkness approximately 45 metres in summer and 100 metres in winter. The greater abundance of aquatic powders during summer, which is the cause of the greater opacity, is due to the thermal stratification of water during the hot season, from which results the power of holding in suspension powders of different densities (*Archives des Sciences physiques et naturelles*, vol. lix. p. 137, 1877).

† *Archives des Sciences physiques et naturelles*, vol. vi. p. 318, 1881.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

FEBRUARY 1885.

XI. *Electromagnets.*—II. *On the Magnetic Permeability of Iron and Steel, with a new Theory of Magnetism.* By R. H. M. BOSANQUET, St. John's College, Oxford.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

BEFORE attempting to draw rigorous experimental conclusions as to the effect of the magnetic properties of Iron and Steel on electromagnets made of these metals, it is necessary to obtain some further information as to the magnetic properties in question.

Hitherto I have accepted the results of Rowland's experiments with rings*; but I have now repeated and varied these experiments, with special regard to the following points:—

To find the average properties assigned by this method to the Iron and Steel I commonly use;

To assign to the results formulæ of a more general and more manageable type than that employed by Rowland;

And to justify experimentally the assumption that the permeability, as calculated from the experiments, is independent of the size and proportions of the rings examined.

A new theory of these magnetic properties will be enunciated and applied.

The iron upon which most of the following experiments have been made is what is called ordinary bar-iron. It is what I commonly use for electromagnets. It is moderately soft and admits of being easily forged. It is stamped with a

* Phil. Mag. [4] xlvi. p. 140 (1873).

crown, and I shall speak of it as crown iron. One ring of best Lowmoor has also been examined.

The steel ring J was forged from the remainder of the bar of cast steel from which the divided magnet was made, which is called A in my paper I. on Permanent Magnets (Phil. Mag. August 1884, p. 142). It was first examined soft, and then after hardening.

The method employed was substantially that of Rowland. The rings were uniformly wound with coils through which the magnetizing current was transmitted. The current employed was a small derived current from the circuit of the dynamo machine; so that it could be opened, closed, or reversed without upsetting the main circuit. The current was measured by means of two galvanometers of Helmholtz's pattern, the one having two coils, the other eighteen. The galvanometers were always erected on the brick stand which forms the standard position for which values of H are determined in the laboratory.

The observations were all made by reversal.

A number of induction-coils, varying from 250 to 1, were wound round the ring to be examined. In circuit with these coils were the ballistic galvanometer and the earth induction-coil, for the reduction of the readings to absolute measure.

Two earth induction-coils were employed. The first had 21 turns, with a mean diameter of 51.30 centim.; the other 250 turns, with a mean circumference of 166.53 centim. With both of these coils the impulse of the galvanometer due to half a turn about a vertical axis in the standard position* was about 7° . But in the case of the larger coil, a deflection corresponds to about twelve times as great an induction as with the smaller coil, so that a very large range is secured. The galvanometer has a circular scale which reads, by reflection, directly to $5'$ and by estimation to single minutes.

The mode of calculation adopted is precisely equivalent to that of Rowland. Rowland's M is current-turns per unit length, $= \frac{Cn}{l}$ in our notation. Then his expression connecting μ , the permeability, with the total number of lines of force is,

$$\text{lines of force} = 4\pi \frac{Cn}{l} \mu (\pi R^2);$$

and for the magnetic induction, or lines of force per unit area,

* The positions of rest between which the half turn is made are such that the plane of the coil is at right angles to the magnetic meridian. Hence the coil cuts the lines of force of H twice in the half turn.

we have

$$\mathfrak{B} = 4\pi \frac{Cn}{l} \mu.$$

Now $4\pi Cn$ is the total magnetic potential exerted on the ring by the magnetizing coil ; whence

$$\frac{\text{potential}}{\mathfrak{B}} = \frac{l}{\mu},$$

which is what I call the magnetic resistance*. μ is then calculated really both by Rowland and myself from the formula

$$\mu = \frac{l\mathfrak{B}}{\text{potential}} = \frac{l}{\text{magnetic resistance}};$$

though of course the form and the letters Rowland uses are different.

The whole process of calculation of an experiment is embodied in the following formulæ.

Let α be the deflection of the ballistic galvanometer due to reversal, with m induction-coils ; then

$$2mS\mathfrak{B} = \kappa\alpha$$

(S = sectional area of bar).

Let β = deflection due to half turn of earth induction-coil (N = number of coils, A = mean area) ; then

$$2NAH = \kappa\beta.$$

Combining these, we have

$$\mathfrak{B} = \frac{\alpha}{\beta} \frac{NAH}{mS}.$$

Let G be that part of the tangent-galvanometer coefficient which is independent of H , so that we have for the current

$$C = GH \tan \delta.$$

Then potential

$$= 4\pi nC = 4\pi nGH \tan \delta,$$

magnetic resistance

$$\rho = \frac{\text{potential}}{\mathfrak{B}},$$

which is independent of H ; and

$$\mu = \frac{l}{\rho},$$

where l is the mean circumference of the ring.

The following Tables exhibit the results of the various

* See Phil. Mag. xvii. p. 531.

experiments thus made. They also show the degree of accuracy attained by the Fourier's series, by which I have endeavoured to represent μ as a function of \mathfrak{B} .

E. *Forged Ring Crown Iron.*

Mean diameter . . $d = 10.035$ centim.

Bar-thickness . . $2R = 1.298$ „

Number of coils . . $n = \begin{cases} 462 \\ 486 \end{cases}$

$$\mu \text{ calc.} = 144 + 221 \cos \theta + 2000 \left\{ \sin \theta + \frac{1}{6} \sin 2\theta + \frac{1}{18} \sin 3\theta + \frac{1}{40} \sin 4\theta \right\},$$

where

$$\theta = \frac{\mathfrak{B}}{100} \text{ degrees.}$$

\mathfrak{B} .	μ .		Diffs.	\mathfrak{B} .	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
129.7	437	437	0	11516	1615	1625	+ 10
239.1	493	498	+ 5	13276	1381	1207	-174
2174.3	1474	1470	- 4	14017	1047	1017	- 30
3186.9	1817	1836	+19	15000	635	732	+ 97
5181.7	2163	2199	+36	16380	218	349	+131
6403.4	2234	2229	- 5	17245	117	119	+ 2
7208.4	2206	2197	- 9				

F. *Forged Ring Crown Iron.*

Mean diameter . . $d = 22.1$ centim.

Bar-thickness . . $2R = 1.292$ „

Number of coils . . $n = 1118$.

$$\mu \text{ calc.} = 200 + 1630 \left\{ \sin \theta + \frac{1}{4} \sin 2\theta + \frac{1}{9} \sin 3\theta + \frac{1}{16} \sin 4\theta \right\},$$

where

$$\theta = \frac{\mathfrak{B}}{100} \text{ degrees.}$$

\mathfrak{B} .	μ .		Diffs.	\mathfrak{B} .	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
15.108	208	208	0	6438.4	1825	1847	+ 22
142.55	360	284	-76	7655.3	1704	1749	+ 45
512.97	587	502	-85	10767	1581	1481	-100
2783.8	1592	1572	-20	12537	1252	1258	+ 6
3616.5	1784	1784	0	13900	1000	989	- 11
4282.6	1869	1858	-11	15025	692	749	+ 57
5746.6	1895	1890	- 5	18834	150	60	- 90

G. *Forged Ring Crown Iron.*Mean diameter . . $d = 21.5$ centim.Bar-thickness . . $2R = 2.535$ „Number of coils . . $n = 989$.

$$\mu \text{ calc.} = 290 \left(1 + \frac{1}{2} \cos \theta\right) + 2000 \left\{\sin \theta + \frac{1}{4} \sin 2\theta\right\},$$

where

$$\theta = \frac{\mathfrak{B}}{100} \text{ degrees.}$$

\mathfrak{B} .	μ .		Diffs.	\mathfrak{B} .	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
50.61	461	461	0	11970	1555	1525	- 30
448.17	765	669	- 96	13710	1192	1047	- 45
1972.5	1615	1419	-196	14426	832	867	+ 35
6363.7	2501	2544	+ 43	15505	664	620	- 44
9003.3	2241	2289	+ 48	16042	395	512	+117
11090	1763	1773	+ 10	17536	145	230	+ 85

H. *Forged Ring Crown Iron.*Mean diameter . . $d = 10.735$ centim.Bar-thickness . . $2R = .7137$ „Number of coils . . $n = 560$.

$$\mu \text{ calc.} = 217 + 164 \cos \theta + 1400 \left\{\sin \theta + \frac{1}{4} \sin 2\theta\right\},$$

where

$$\theta = \frac{\mathfrak{B}}{100} \text{ degrees.}$$

\mathfrak{B} .	μ .		Diffs.	\mathfrak{B} .	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
34.375	395	394	- 1	11261	1134	1198	+ 64
286.85	418	486	+ 68	13355	885	769	-116
1079.4	817	769	- 48	14501	630	557	- 73
6025.5	1797	1816	+ 19	14718	502	519	+ 17
7704.6	1717	1771	+ 54	15409	254	407	+153
8435.7	1710	1681	- 29	17642	97	98	+ 1
9739.6	1354	1498	+144				

K. Forged Ring Crown Iron.

Mean diameter . . $d = 22.725$ centim.Bar-thickness . . $2R = .7544$ „Number of coils . . $n = 1217$.

$$\mu \text{ calc.} = 380 \cos \theta - 30 + 1770 \left\{ \sin \theta + \frac{1}{4} \sin 2\theta \right\},$$

where

$$\theta = \frac{\mathfrak{B}}{100} \text{ degrees.}$$

\mathfrak{B} .	μ .		Diffs.	\mathfrak{B} .	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
67.293	422	381	- 41	9598.2	1557	1660	+103
293.43	428	486	+ 58	10413	1531	1384	-147
2174.7	1288	1278	- 10	11511	1104	1357	+253
2337.5	1176	1343	+167	12148	799	953	+154
3949.0	1766	1823	+ 57	13246	666	569	- 97
4710.5	1982	1967	- 15	13104	505	618	+113
5719.9	2070	2067	- 3	13671	358	466	+108
8677.4	1914	1808	-106	15053	135	131	- 4
8889.8	1775	1764	- 11				

I. Forged Ring Best Lowmoor Iron.

Mean diameter . . $d = 10.025$ centim.Bar-thickness . . $2R = 1.293$ „Number of coils . . $n = \begin{cases} 470 \\ 476 \end{cases}$

$$\mu \text{ calc.} = 250 + 1800 \left\{ \sin \theta + \frac{1}{4} \sin 2\theta + \frac{1}{9} \sin 3\theta + \frac{1}{16} \sin 4\theta \right\},$$

where

$$\theta = \frac{\mathfrak{B}}{100} \text{ degrees.}$$

\mathfrak{B} .	μ .		Diffs.	\mathfrak{B} .	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
32.944	271	272	+ 1	7090.9	2173	2012	-161
429.15	617	547	- 70	8193.9	1935	1915	- 20
1321.8	1024	1078	+ 54	9690.8	1842	1795	- 47
1623.6	1427	1247	-180	10400	1483	1730	+247
2395.1	1558	1616	+ 58	13159	1188	1390	+202
4541.5	2072	2116	+ 44	15050	818	851	+ 33
6324.2	2080	2077	- 3	16309	592	57	- 18
6712.3	1993	2045	+ 52	16939	444	448	+ 4
6700.1	2033	2046	+ 13	19303	155	182	+ 37

*J. Forged Ring Cast Steel (Soft).*Mean diameter . . $d = 14.25$ centim.Bar-thickness . . $2R = 1.421$ „Number of coils . . $n = 1074$.

$$\mu \text{ calc.} = 120 + 340 \sin \theta,$$

where

$$\theta = \frac{B}{100} \text{ degrees.}$$

B.	μ .		Diffs.	B.	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
21.873	126	121	- 5	5815.9	420	409	- 11
83.360	120	125	+ 5	9467.8	461	459	- 2
277.3	156	136	-20	11497	443	428	- 15
1825.9	248	226	-22	12443	326	400	+ 74
2367.0	249	256	+ 7	15352	154	272	+118
3516.6	314	312	- 2	18597	90	85	- 5

*J. Forged Ring Cast Steel (Hard).*Mean diameter . . $d = 14.25$ centim.Bar-thickness . . $2R = 1.421$ „Number of coils . . $n = 1069$.

$$\mu \text{ calc.} = 45 + 90\{\sin \theta + \frac{1}{2} \sin 2\theta\},$$

where

$$\theta = \frac{B}{100} \text{ degrees.}$$

B.	μ .		Diffs.	B.	μ .		Diffs.
	Obs.	Calc.			Obs.	Calc.	
12.75	49	45	-4	645	67	62	- 5
59.48	47	47	0	2189	115	110	- 5
120.8	48	48	0	4492	150	153	- 3
170.4	51	52	+1	6496	157	161	+ 4
267.9	57	53	-4	8959	134	135	+ 1
405	62	59	-3	13870	70	60	-10
551	64	62	-2				

First, as to the saturation-value. In nearly all the rings of crown iron the saturation-value of the induction is under 18,000, although in ring F it appears to exceed 19,000. This agrees generally with Rowland's result as to rings.

In my paper on Electromagnets (Phil. Mag. xvii. p. 532) I have shown that in the case of the two bars there examined the saturation-values were higher, and did not appear to give evidence of having any fixed limit. This seems intelligible

when we remember that in the bar the lines of force are crowded closely only at the equatorial section.

The formula by which Rowland represented his results was of the type

$$\mu = A \sin \left\{ \frac{\mathfrak{B} + a\mu + C}{D} \right\},$$

in which A, C, D, and a are constants depending upon the kind and quality of the metal used. This type of formula is unmanageable; and it seemed to me necessary to attempt a more direct expression of the facts.

Rowland's formula offers the suggestion that we may regard values of μ which occur in these experiments as corresponding to the first half of a periodic change, the whole of which would be completed for a value of \mathfrak{B} corresponding to about twice the saturation-value. This being so, it is theoretically possible, according to Fourier's theorem, to express any set of the experimental values of μ by a series of sines and cosines of an angle proportional to the induction and of the multiples of that angle; and the only difficulty is to find the coefficients of the different terms of the series.

Unfortunately the distribution of the values obtained does not admit of the application of the simplest form of harmonic analysis; so that the accurate determination of the coefficients is attended with a good deal of difficulty; and after trial of various methods of solution of equations, including an extended application of the method of least squares, it was found that a process of trial and error was capable of giving better results than could be obtained in any other way.

The representations thus obtained are not in all cases very close, but they are quite sufficient to show, by comparison of the different rings of the same iron, the uselessness of attempting to define minutely the properties of a given kind of iron. For the analysis of the different sets these are as valuable, on account of the clearness of the expressions, as the representations founded on my subsequent theory, which are as close or closer.

As to the size of the rings. There are five rings of crown iron, E, F, G, H, K. Of these, E, H have approximately the same mean diameter; F, G, K have mean diameters rather more than twice as great. H, K have about the same bar-thickness, E, F bar-thickness nearly twice as great, and G a bar-thickness between three and four times as great. But when we examine the expressions for μ , we fail to find any systematic differences which appear to correspond with these differences in the dimensions. So that, so far as this small number of experiments goes, the fundamental suppositions are

justified in leading to values of μ independent of the dimensions of the rings.

It is a question of particular interest whether the iron in the interior of thick bars is in any degree shielded by that which encloses it from the effects of magnetic potential. In rings we have this question detached from the complications caused by the ends of straight bars or tubes, and are face to face with the simple question:—Is magnetic potential so absorbed in its passage through external layers of iron that its effect in the interior is sensibly diminished?

To answer this question it is convenient to make use of the ordinary expression for magnetizing force ($= \frac{\mathfrak{B}}{\mu}$ in the above tables), and to tabulate the values of the induction in the different rings corresponding to a series of definite values of the magnetizing-force. If, then, there is absorption of the force in the external layers, the inside of the thickest rings should be the most shielded, and the induction should throughout be lower in those rings for the same magnetizing force.

An inspection of the following Table will show that, if anything, the reverse of this is the case.

Table of Values of \mathfrak{B} in the different Crown Iron Rings, corresponding to certain Values of the Magnetizing Force.

	G	E	F	H	K
Mean diam....	21.5 cm.	10.035 cm.	22.1 cm.	10.735 cm.	22.725 cm.
Bar-thickness.	2.535	1.298	1.292	.7137	.7544
Magnetizing Force.					
.2	126	73	65.3	82.4	85
.5	377	270	224	208	214
1	1449	1293	840	675	885
2	4564	3952	3533	2777	2417
5	9900	9147	8293	8479	8884
10	13023	13357	12540	11376	11388
20	14911	14653	14710	14066	13273
50	16217	15704	16062	15174	13890
100	17148	16677	17900	16134	14837

The thickest ring, G, has the highest inductions throughout. The thinnest rings fall a little behind on the whole, except in the initial values. It seems probable that this may be due to slight flaws of small surface-extent, which may not perceptibly impair the magnetic conductivity of the thick ring, but may interfere with that of the thin ring. At all events the hypothesis of shielding is conclusively negatived.

Returning to the general tables, the variations which strike

us most are those in the initial values of μ and those in the coefficient of $\sin \theta$. We can only conclude, either that the iron is of very variable quality, or that the forging of the rings is liable to introduce these large discrepancies.

I may mention here that, in accordance with Rowland's direction, a coating of at least $\frac{1}{8}$ of an inch was removed in the lathe from the rings after forging. The ring K was turned out of the very heart of a forged ring originally nearly four times its ultimate thickness.

The Lowmoor iron ring shows low initial permeability and a high saturation-point. In both respects it much resembles ring F, but the maximum permeability of the Lowmoor is greater than that of F, though considerably less than that of E or G; in fact the differences due to the type of iron appear inconsiderable compared with the differences between different rings of the same iron.

The soft steel ring presents very remarkable results. First we notice that the saturation-point is decidedly high; that is to say, by the employment of sufficient force, as shown by the low values of the permeability, the induction in the soft steel was actually raised to over 18,000.

The initial permeability is about one third of the average value for soft iron; the maximum permeability about one fifth of the average value for soft iron. But the strangest thing of all is that the variable part of the permeability appears to be completely represented by a single term involving the sine of the angle proportional to the induction, so that the maximum permeability corresponds to an induction of about 9000 instead of to an induction of between 5000 and 6000 as is usual in soft iron.

In the case of the hard steel ring the power employed was insufficient to force the steel up to its saturation-point, and before the ring could be re-examined with a larger number of windings the tubes of the boiler broke down and put a stop to work for the present. But the observations already obtained are very fairly represented by a formula which would give a high saturation-point. The initial permeability is less than one half that of the soft steel. The maximum permeability is about one third that of the soft steel, and it corresponds to a somewhat lower value of the induction than in soft iron, instead of to a higher value as in soft steel; that is to say, what we may call the octave term ($\sin 2\theta$), which vanishes for the soft steel, has for the hard steel the largest proportional value that occurs in any of the rings examined. The physical meaning of this will be given us by my new theory.

In Rowland's paper above referred to he observes that it is

probable that Weber's theory may be so modified as to give an equation of the type he employs. I am not aware that this has been attempted, and it seems improbable on account of the nature of the equation. Further it is clear that the functions involved are continuous through the whole range of the magnetization. And the essence of Weber's theory and of all modifications of it is to divide the course of events into separate portions, in which different causes are supposed to operate, and different expressions are obtained for the results.

The statement that the results are continuous applies to the residual subpermanent magnetism as well as to that obtained by reversal; for although I have not made any determinations of residual magnetism, the data in Rowland's table I. are sufficient to give a series of values of residual magnetism which determine the point. Using the same angle proportional to the total induction for θ , we can find a new quantity μ' , which we may call susceptibility to residual magnetism.

The following table gives the results of this process as applied to Rowland's table I.:—

\mathfrak{B} or Q , total induction by reversal.

\mathfrak{B}' or P , subpermanent or residual induction ($Q - T$).

μ , Rowland's value of the permeability.

Then, if $\mu' = \frac{l \mathfrak{B}'}{\text{potential}}$ in the same way that $\mu = \frac{l \mathfrak{B}}{\text{potential}}$,

$$\mu' = \mu \frac{\mathfrak{B}'}{\mathfrak{B}},$$

from which the following values are calculated:—

Initial values $\left\{ \begin{array}{l} \mu_0 = 332 \\ \mu'_0 = 69 \end{array} \right\}$ from first and third lines, by differences.

μ' or Susceptibility to Subpermanent Magnetism, from Rowland's table I.

\mathfrak{B} .	\mathfrak{B}' .	μ .	μ' .	\mathfrak{B} .	\mathfrak{B}' .	μ .	μ' .
Q .	P .			Q .	P .		
71.5	18.7	390.7	102.7	8943	6369	2208	1572
600.5	211.1	868.7	305.4	10080	6838	1899	1288
966.7	438.7	1129	512.4	12270	7502	1448	885
2460	1572	1936	1237	12970	7666	1269	750
2923	1942	2078	1380	13630	7520	1137	627
3082	2064	2124	1422	14540	7989	824.1	452.8
4959	3628	2433	1780	15770	8116	461.8	237.6
5482	3811	2470	1717	16270	7852	353.8	170.7
5782	4011	2472	1715	16600	7888	258	122.6
6651	4746	2448	1747	17500			
7473	5434	2367	1721				

There is a slight discontinuity here near the maximum value of μ' ; but nothing to obscure the general applicability of a continuous formula.

We note that the initial value of μ' , though small, is by no means evanescent; that the curve formed by μ' as ordinate and \mathcal{B} as abscissa rises very steeply at starting; that μ' has a maximum value equal to nearly two thirds of the maximum value of μ ; and that, although the position of this maximum of μ' is somewhat obscured by the small discontinuity above referred to, yet it is substantially in the same position as the maximum of μ .

On the whole, then, the facts are incompatible with those with which Weber's theory and Maxwell's modification of it are framed to correspond; and it will be of interest to consider whether there be any hypothesis by which these facts can be represented.

Prof. Hughes has recently put forward a theory, the fundamental supposition of which appears to be identical with that of the theory of Weber; but I cannot find that it has been applied so as to give any quantitative account of magnetizing functions or permeabilities.

I will now give an illustration of the suppositions as to molecular magnets, which would have to be made, in order that the principal term of the permeability may be proportional to the sine of an angle whose zero corresponds approximately to the zero of magnetic induction, and 180° to saturation, the angle being proportional to magnetic induction in between. The resulting theory is of an impossible character.

Let a typical molecule be represented by a small magnet hung by a torsion-wire at the centre of a coil, and let the magnet be placed so that when no current is passing it stands nearly at right angles to the plane of the coil, but with the poles in the opposite direction to that which the action of the coil tends to make them assume. Suppose, then, that a current is sent through the coil; the needle will be deflected, and will rest at the point where the deflecting couple on the needle is balanced by the torsion of the wire. Now the couple due to the coil may be expressed as $GC \sin \theta$; where C is the current, G a constant, and θ the deflection from a position at right angles to the coil. The torsion may be represented by $\tau(\theta - \alpha)$, where α is small; or approximately by $\tau\theta$, whence we have for the position of rest

$$\tau\theta = GC \sin \theta.$$

Let θ , or the rotation, represent the magnetic induction. It is clear that, however great C may be, it can never tend to increase θ beyond 180° , since this is the position in which the coil tends to place the needle independently of the torsion: this represents saturation. Further, C is proportional to the magnetic potential or magnetizing force, so that $\frac{\theta}{C}$ is proportional to μ and $\frac{\theta}{C} = \frac{G}{\tau} \sin \theta$; so that μ is represented approximately by a quantity proportional to the sine of an angle proportional to the induction.

If the fundamental ideas of this illustration could be in any way applied to the behaviour of systems of molecules, it would be easy, by small modifications of the principal suppositions, to take count of the smaller terms; but it seems quite impossible to carry out this application; and the illustration is only useful as showing the sort of circumstances which would have to be imagined in order to obtain a formula of this sort from the hypothesis of molecular magnets.

I will proceed to sketch another theory which appears to me more reasonable, and leads to forms of function by which we can represent the experiments very well.

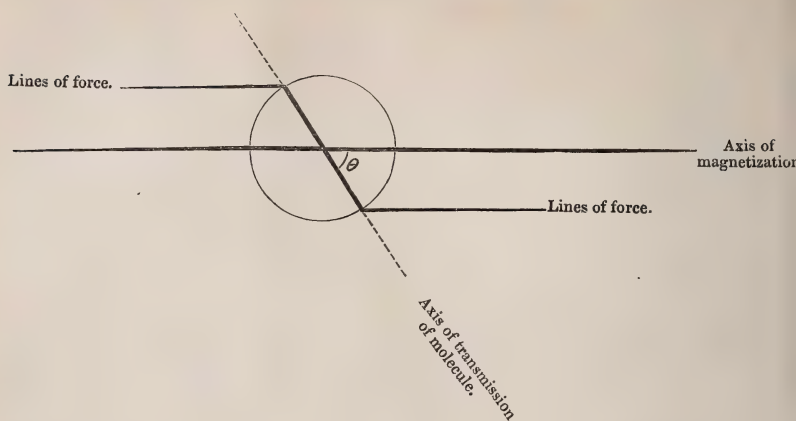
Suppose that every molecule of iron has one axis, and only one axis, through which magnetism can be transmitted.

The molecules in an inert mass of iron are supposed to have their axes of transmission distributed in all directions. There will then be a very small proportion of molecules whose axes are so situated as to form continuous lines of transmissibility for magnetic lines of force proceeding in a given direction. Thus, for small magnetizing forces there will be a small transmissibility or permeability.

In the case in which molecules are so situated that the extremities of their axes do not join, the magnetic lines of force have to pass between the molecules as they would through ordinary space. We suppose that the transmissibility through the axes of the molecules is very great compared with that in ordinary space or in the interstices of the molecules.

As the magnetic induction increases, couples are established by the action of the lines of force, which tend to move the axes of the molecules towards the direction of the lines of force.

Consider a single molecule. Let the plane of the paper cut it in the plane containing its magnetic transmission axis and the axis of magnetization. Then the lines of force will traverse the molecule somewhat in the way shown in the



above figure, though they will only be parallel on the average to the mean axis of magnetization, where they issue from the molecule.

Now it is known that many magnetic phenomena may be expressed by the hypothesis of a tension along the lines of force. If we suppose such a tension to exist here, we have a couple whose force is equal to the tension, and arm equal to the diameter of the molecule $\times \sin \theta$, tending to turn the axis of the molecule towards the axis of magnetization. If we suppose the molecules to give way to this force, the ends of the axes of the successive molecules approach each other, and the intermediate space, as well as the number of molecules, required to be traversed is diminished. This accounts for the increase of the permeability in the first part of the range of the induction.

Without attempting to frame exact hypotheses we can see generally that the whole magnetic resistance may be expressed by supposing the lines of force to form a zigzag, the obliquity of which diminishes as the tension of the lines of force increases. From this representation, assuming a law similar to Ohm's law for electric circuits, we get the expression

$\frac{l}{\cos \delta}$ as representing the reduced resistance of a length l of

the magnetic substance, δ being an auxiliary angle representing the mean obliquity of the zigzag. We have further to take into account the fact that all forms of iron and steel are capable of transmitting only a limited amount of magnetic induction through a given area. In the absence of any accurate knowledge as to the law which governs this part of the resistance of the molecules to the lines of force, I shall

assume a factor in the permeability which vanishes for the saturation-value, namely $(\mathfrak{B}_\infty - \mathfrak{B})$, where \mathfrak{B}_∞ is the saturation-value of the induction. The resistance will then be proportional to the reduced length divided by this factor, so that

$$\text{resistance} = \frac{l}{\mu}$$

$$= \frac{l}{A(\mathfrak{B}_\infty - \mathfrak{B}) \cos \delta},$$

or
$$\mu = A(\mathfrak{B}_\infty - \mathfrak{B}) \cos \delta.$$

We may give some further account of the factor $(\mathfrak{B}_\infty - \mathfrak{B})$ based on an analogy between the lines of magnetic force and solid wires. If a number of thin wires had to be packed into a cylindrical tube, by the time there were a certain number of them the hollow of the tube would be full, and no more could be got in. Also, if there were a less number, the space available in the tube would be measured by the number of wires that could still be added.

Now if our axis of transmission in the molecule represent the hollow tube, and the lines of force the wires, it is reasonable to assume that the permeability is proportional to the portion of the channel left open and unoccupied. And this is precisely given by $(\mathfrak{B}_\infty - \mathfrak{B})$. It is only an analogy, but it seems to me not destitute of force. We may call $(\mathfrak{B}_\infty - \mathfrak{B})$ "defect of saturation."

Let us return to the consideration of the equilibrium of the molecule. On the one hand we have the couple exerted by the lines of force; on the other that arising from molecular attachments, which tends in the opposite direction.

The simplest law of molecular attachments is the law of torsion; and in considering a single molecule I assume that the couple tending to restore it to its place is proportional to the angular rotation from the position of rest. (The displacements of subpermanent and permanent magnetism are at first left out of the question.) If therefore

ρ be the diameter of the molecule,

k the torsion per degree,

ϕ displacement from rest,

θ inclination to direction of magnetization in disturbed position,

$$\mathfrak{B}^* \rho \sin \theta = k \omega \phi$$

* I assume that the tension along the lines of force is proportional to \mathfrak{B} instead of to \mathfrak{B}_2 according to Maxwell. I shall return to the point; but may say here that the assumption of force proportional to \mathfrak{B}_2 led to formulæ which failed to represent the experiments.

expresses the condition of equilibrium ; ω is the unit angle, so that $\omega\phi$ is ϕ expressed in degrees.

If we pass from the consideration of a single molecule to that of a molecular arrangement we are confronted with problems which, for the most part, must be regarded as insoluble. One point, however, is clear.

If the axes of the molecules are arranged uniformly in all directions, the average inclination to any one line is 60° .

For we may suppose the axes of transmissibility uniformly distributed over the surface of each hemisphere, and the hemisphere is divided into two equal portions by a cone whose semi-vertical angle is 60° .

If therefore we represent the condition of the average molecule with respect to inclination by a single molecule, we must assume the initial inclination to the axis of magnetization to be 60° .

And, in the above expression, when $\theta=0$, $\omega\phi=60^\circ$; so that $\omega\phi=60^\circ-\omega\theta$. And the equation becomes,

$$\mathfrak{B} = \frac{k}{\rho} \frac{60^\circ - \omega\theta}{\sin \theta}.$$

It only remains to connect θ with the auxiliary angle δ . In the absence of exact knowledge of the molecular arrangements, the only thing we can do is to assume the simplest connection possible. We shall assume

$$\delta = f\theta,$$

where f is a factor. We thus obtain the system of equations,

$$\mu = A(\mathfrak{B}_\infty - \mathfrak{B}) \cos \delta,$$

$$\delta = f\theta,$$

$$\mathfrak{B} = \frac{k}{\rho} \frac{60^\circ - \omega\theta}{\sin \theta}.$$

This system of equations involves only the arbitrary constants A , \mathfrak{B}_∞ , f , and $\frac{k}{\rho}$, for 60° cannot be called arbitrary. The equations are capable of representing very well the connection between μ and \mathfrak{B} . The Fourier's series are in some respects more flexible. But these equations are important as regards their physical bearing. As the computations present some difficulty, I here give a systematic scheme for adapting these equations to the representation of experimental series of values of \mathfrak{B} and μ .

Given a series of experimental values of \mathfrak{B} and μ , it is required to represent them by the formulæ

$$\mu = A(\mathfrak{B}_\infty - \mathfrak{B}) \cos \delta, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\delta = f\theta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\mathfrak{B} = \frac{k}{\rho} \frac{60^\circ - \theta \omega}{\sin \theta}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Let \mathfrak{B}_1, μ_1 be the lowest pair of values given ;

\mathfrak{B}_2, μ_2 those corresponding to μ_2 , the maximum value of μ ;

\mathfrak{B}_∞ the saturation-value of \mathfrak{B} as estimated from the experiments or assumed.

The first process aims at determining A by trial and error, under the condition that the maximum value of μ represented by the formulæ shall belong to the value \mathfrak{B}_2 given by experiment. The theory of this is as follows.

Since μ is a maximum, we have from (1),

$$d\mu = 0 = -d\mathfrak{B} \cos \delta - (\mathfrak{B}_\infty - \mathfrak{B}) \sin \delta d\delta,$$

or

$$\tan \delta = - \frac{\frac{d\mathfrak{B}}{d\delta}}{(\mathfrak{B}_\infty - \mathfrak{B})};$$

whence from (2),

$$\tan \delta = - \frac{1}{f} \frac{\frac{d\mathfrak{B}}{d\theta}}{(\mathfrak{B}_\infty - \mathfrak{B})}. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Again, differentiating (3),

$$d\mathfrak{B} \sin \theta + \mathfrak{B} \cos \theta d\theta = - \frac{\omega k}{\rho} d\theta,$$

or

$$\frac{d\mathfrak{B}}{d\theta} = - \frac{\mathfrak{B} + \frac{\omega k}{\rho} \sec \theta}{\tan \theta}. \quad . \quad . \quad . \quad . \quad (5)$$

Combining (4) and (5),

$$\tan \delta = \frac{1}{f} \frac{\mathfrak{B} + \frac{\omega k}{\rho} \sec \theta}{\tan \theta (\mathfrak{B}_\infty - \mathfrak{B})}, \quad . \quad . \quad . \quad . \quad (6)$$

which is the condition for the maximum value of μ .

The work proceeds as follows:—

Assume a value of A , (A').

$$\text{Find } \omega \delta_1 \text{ from } \cos \delta_1 = \frac{\mu_1}{A'(\mathfrak{B}_\infty - \mathfrak{B}_1)},$$

$$\omega \delta_2 \text{ from } \cos \delta_2 = \frac{\mu_2}{A'(\mathfrak{B}_\infty - \mathfrak{B}_2)}$$

($\omega \times$ angle is used throughout to represent degrees, ω being $=57^\circ.296$).

Let first approximation be denoted by one accent, second by two, and so on.

Assume $\omega\theta_1' = 60^\circ$. Then

$$f' = \frac{\omega\delta_1}{60^\circ};$$

whence

$$\omega\theta_2' = \frac{\omega\delta_2}{f'}.$$

$$\frac{k'}{\rho} = \frac{\mathfrak{B}_2 \sin \theta_2'}{60^\circ - \omega\theta_2'}.$$

Second approximation.

$$\omega\theta_1'' = 60^\circ - \frac{\sin \theta_1' \mathfrak{B}_1}{\frac{k'}{\rho}},$$

$$f'' = \frac{\omega\delta_1}{\omega\theta_1''};$$

whence

$$\omega\theta_2'' = \frac{\omega\delta_2}{f''},$$

$$\frac{k''}{\rho} = \frac{\mathfrak{B}_2 \sin \theta_2''}{60^\circ - \omega\theta_2''}.$$

In general this will be enough, but there is no difficulty in repeating the process as often as necessary.

Dropping the accents, we have for calculating the right-hand side of (6),

$$\mathfrak{B}_2, \mu_2, \omega\theta_2, f, \frac{k}{\rho}.$$

From the result find $\tan \delta$, whence δ , or rather $\omega\delta$.

If the condition as to the maximum is satisfied by the assumed value of A , this $\omega\delta$ will coincide with $\omega\delta_2$. If they differ, as they generally will, the assumed value of A is wrong. If $\omega(\delta - \delta_2)'$ is positive, A has to be increased, and *vice versa*.

Another hypothesis as to the value of A has then to be made, and the work repeated, (A'').

The final value $\omega(\theta - \theta_2)''$ thus obtained, compared with $\omega(\theta - \theta_2)'$, offers a means of estimating the true value of A , for which the maximum of μ given by the equations corresponds to the proper value of \mathfrak{B} . But the convergence of the process is not quick enough to enable differences to be used for the estimation, unless close approximation has been already

attained. In this case A may be estimated from the formula

$$A' - (A' - A'') \frac{\omega(\delta - \delta_2)'}{\omega(\delta - \delta_2)' - \omega(\delta - \delta_2)''},$$

where one accent refers to the first of two assumptions, two to the second.

Before this way of estimating A was devised, the only way of approximating to it was to carry out the whole calculation of μ from \mathfrak{B} for each assumed value of A , and then judge from the results. It was soon seen that the representations obtained differed in the position of the maximum of μ . The following correspondences were thus obtained for ring E:—

Assumed A .	Value of \mathfrak{B} for which μ is a maximum.
1 . . .	Between 7000 and 11,000.
$\frac{1}{2}$. . .	About 7000, still too late.
$\frac{1}{3}$. . .	Before 6400, a little too early.

An accurate judgment could be formed by the correspondences of μ before and behind the maximum.

The application of the above method saved the great labour of these complete computations by way of trial.

It is necessary to repeat the above processes until the resulting value of $\omega(\delta - \delta_2)$ is sensibly zero. The quantities f and $\frac{k}{\rho}$ obtained in the computation which satisfies this condition are taken on for the final process.

The next thing is to form the table connecting \mathfrak{B} and θ , from equation (3). This is most conveniently done by forming a table of $\log \frac{60^\circ - \omega\theta}{\sin \theta}$ once for all, for each degree from about 20° to 60° *. The addition of these logarithms to that of $\frac{k}{\rho}$ gives the values of \mathfrak{B} corresponding to the degrees of $\omega\theta$.

This table (\mathfrak{B}, θ) is written out, with differences. The values of $\omega\theta$ corresponding to the experimental values of \mathfrak{B} are then found by interpolation.

Multiplying $\omega\theta$ by f , we find $\omega\delta$ corresponding to each experimental value of \mathfrak{B} .

With A , $\omega\delta$, and \mathfrak{B} , μ is calculated from equation (1) for each experimental number \mathfrak{B} .

The following are comparisons of this theory with experiment.

* For steel the table is formed for every $10''$ from 60° to $59^\circ 59'$, and then for every minute down to 59° .

Equations.

$$\mu = A(\mathfrak{B}_{\infty} - \mathfrak{B}) \cos \delta, \quad \delta = f\theta, \quad \mathfrak{B} = \frac{k}{\rho} \frac{60^\circ - \omega\theta}{\sin \theta}.$$

Ring E. $\mu = \cdot 3856 (17,500 - \mathfrak{B}) \cos \delta,$

$$\log \frac{k}{\rho} = 2.32208, \quad \log f = \cdot 16153.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
129.7	59° 28'	86° 16'	436	437	- 1
239.1	59 2	86 4	457	493	- 36
2174.3	51 51	75 13	1508	1474	+ 34
3187	48 36	70 30	1842	1817	+ 25
5182	43 7	62 33	2190	2163	+ 27
6403	40 17	58 26	2235	2234	+ 1
7208	38 37	56 10	2210	2206	+ 4
11516	31 25	45 34	1615	1615	0
13276	29 11	42 20	1204	1382	-178
14017	28 20	41 6	1012	1047	- 35
15000	27 16	40 8	737	635	+102
16380	25 55	37 25	342	218	+124
17245	24 53	36 6	79	117	- 38

J. Soft Steel. $\mu = 11 (20,000 - \mathfrak{B}) \cos \delta,$

$$\log \frac{k}{\rho} = 4.78936, \quad \log f = \cdot 17595.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
57.6	59° 59' 56.7"	89° 58' 5"	122.3	123	- .7
277	59 59 45.7	89 57 50	137	156	- 19
1826	59 58 28	89 55 52	240	248	- 8
2367	59 58 00	89 55 16	267	249	+ 18
3517	59 57 2	89 53 46	329	314	+ 15
5816	59 55 6	89 50 53	414	420	- 6
9468	59 52 2	89 46 19	461	461	0
11497	59 50 19	89 43 41	444	443	+ 1
12443	59 49 31	89 42 29	424	326	+102
15352	59 47 5	89 38 53	314	154	+160
18597	59 44 20	89 34 41	114	90	+ 24

Here \mathfrak{B}_{∞} is a little overestimated. Probably 19,500 would be nearer the mark.

A is the permeability of the molecules themselves per unit defect of saturation. For, putting $\theta = \delta = 0$, or supposing the

axes all arranged in continuous chains parallel to the axis of magnetization, then

$$A = \frac{\mu}{\mathfrak{B}_{\infty} - \mathfrak{B}}.$$

The molecular permeability for given \mathfrak{B} , under these circumstances, is therefore

$$\mu = A(\mathfrak{B}_{\infty} - \mathfrak{B}),$$

which increases continually as \mathfrak{B} diminishes, and has the maximum values ($\mathfrak{B}=0$),

$$\begin{array}{ll} \text{Ring E} & \mu = 6748, \\ \text{J (soft steel)} & \mu = 220,000. \end{array}$$

Nothing is more surprising in these results than the enormous values of the molecular permeability of the soft steel, and the way in which the effect of this is suppressed by the comparative immobility of the molecules. Thus:—

	Extreme variation of average inclination:	Extreme rota- tion of average molecule.
Soft iron ring E	60° 24° 53'	33° 7'
Soft steel J	60° 59° 44' 20''	15' 40''

The molecular permeability of the soft steel is 28·5 times as great as that of the soft iron. The extreme rotation of the average molecule is 127 times as great in the soft iron as in the soft steel.

$\frac{k}{\rho}$ is 287 times as great in the soft steel as in the soft iron; so that, assuming that the mean diameter of the molecules is the same, the force of torsion arising from molecular attachment has this ratio in the two cases.

Putting together the conditions associated with early and late maxima in the Fourier's-series expressions and those of the present theory, we have

	Early Maxima.	Late Maxima.
Fourier's series.	{ Octave and higher terms well developed.	{ Octave and higher terms small or absent.
New magnetic equations.	{ . A small.	{ A large.

Recalling the meaning of A , we see that the development of the octave and higher terms in the Fourier's series corresponds to low molecular permeability, other things being equal*. It

* A depends on the absolute magnitudes of the μ 's to be represented as well as on their course.

will be of interest to see, when the hard-steel experiments are complete, how far the resulting inference of low molecular permeability in that case is borne out.

As to the question whether the tension of the lines of force is proportional to \mathfrak{B} or to \mathfrak{B}_2 , it is a question for experiment, and I shall take an early opportunity of endeavouring to decide it. But in the mean time it appears quite clear that if lines of force mean anything, it is that their number is proportional to the force. Consider a magnet-pole. We may represent the distribution of force about it either by the law of the inverse square of the distance, or by a system of lines of force radiating from the pole. If the two are equivalent, it involves the consequence that the force at any point is proportional to the number of lines of force.

XII. *The Influence of an Electric Current in Modifying the Rate of Thinning of a Liquid Film.* By Prof. A. W. REINOLD, F.R.S., and Prof. A. W. RÜCKER, F.R.S.*

IN 1877 the results of some experiments made by us on liquid films were published in the Proceedings of the Royal Society (Proc. Roy. Soc., No. 182, 1877). One object of the investigation was the determination of the electrical resistance of the films. The method of Wheatstone's Bridge was employed for this purpose, and the current was passed through a film only at the moment when an observation was required. Under these circumstances the films generally thinned until they became black; and we succeeded in obtaining several measures of their resistance when this colour was displayed. Subsequently this method was abandoned for another, in which continuous currents were passed through the film, and the difference of potential between two fine wires thrust into it was measured by an electrometer, and compared with that between two other points in the same circuit separated by a known resistance†.

Although in many respects a great improvement, this method was in one point inferior to that previously employed. The behaviour of the films was very irregular when compared with that of those previously examined by the galvanometer, although they were formed of a liquid having the same composition. Sometimes they thinned rapidly, sometimes slowly;

* Communicated by the Physical Society. Read December 13, 1884.

† "On the Electrical Resistance of Thin Liquid Films, with a Revision of Newton's Table of Colours," Phil. Trans. 1831.

at one time the black would appear after a comparatively short interval; at another, the film would persist for many hours without showing a trace of black. As at this time our attention had not been directed to the possible influence of the current itself in modifying the rate of thinning of the film, no special precautions were taken with regard to the current, either as to its direction or intermittence; but we now know that the apparently capricious behaviour of the films observed on many occasions was largely, if not entirely, due to want of method in managing the current.

We have recently made some experiments with the definite object of determining the effect upon a liquid film of passing through it a current of electricity. Some of the results obtained were communicated to Section A of the British Association at the Montreal meeting. We propose to give in this paper a somewhat more detailed account of our observations.

The liquid employed consisted either of a solution of potash soap in water, or of Plateau's *liquide glycérique*, containing a certain proportion of nitre to increase its conductivity. The films had the form of vertical cylinders, the upper and lower ring supports being of platinum, and about 33 millim. in diameter. The cylinders were generally either 30 or 40 millim. long. Occasionally other lengths were employed. Three films were under examination at the same time; two being in one glass box, the third in a separate glass box. The two that were together were supported by platinum rings identically alike and sharply bevelled at the edge; the edges of the supports of the third were much thicker and were rounded. We had thus the means of ascertaining whether any difference in the behaviour of the films was due to the form or thickness of their supports. No such difference was observed.

Each of the film-boxes was placed in the centre of a water-tank with glass sides, in order to prevent rapid changes of temperature. In all cases the films were surrounded with air saturated with the vapour of the liquid of which they were made. The constancy of the hygrometric state of the air, and that of its temperature were indicated respectively by a hair-hygrometer and a thermometer inside the glass case.

The films were blown with air which was first dried and then passed over some of the soap-solution in a wide tube. The state of the air inside the film was thus approximately the same as that on the outside.

In the earlier experiments a battery of nine Leclanché cells was used, the electromotive force of which was about 12 volts.

More recently we have employed the current from a Siemens' dynamo. The current passed through a sensitive reflecting galvanometer, the deflection on the scale measuring approximately the strength of the current. This varied from about 100 microampères to half a microampère.

We began our experiments by observing the behaviour of a number of films, each 40 millim. long, formed of potash-soap solution without any glycerine, which were allowed to thin in the ordinary way, no current passing through them. In less than a minute after the adjustment of the film to the cylindrical form narrow rings of colour appeared, and white of the first order of Newton's rings was soon seen at the top, generally bordered by a narrow band of deep blue, the blue of the second order. The coloured bands did not extend beyond 10 millim. from the top, the rest of the film being colourless. This was the state of the case at the end of five or six minutes. In from eight to fifteen minutes from the moment of formation of the film a black ring appeared and slowly extended downwards. Nine films were examined, and all behaved substantially in the same way.

We now tried the effect of passing a current through the films. It is unnecessary to describe all the observations that were made, as the object of many of them was merely to obtain confirmation of previous results. A few typical cases may be described.

i. *Downward Current. Plain Potash-soap Solution.*

(1) Film 40 millim. long. A current was passed through from the moment of its formation. Broad bands of colour formed and spread with great rapidity, soon occupying the whole area of the film. After six minutes the current was 2.6 microampères (m.a.). In eleven minutes there was a ring of black, which in two minutes more increased to 3 millim. The current was then stopped.

It appears from this and a number of other experiments made in the same way that the effect of the downward current is to promote the thinning of the film. The bands are broader and spread out more rapidly than when no current is applied.

(2) A film 40 millim. long had thinned until 2.5 millim. of black were formed. Next to the black came a band of deep blue. A downward current of 5.18 m.a. was applied. The blue changed to white, the black not being altered. After nine minutes the black was still of about the same breadth and the white had increased to 16 millim., the current being now 4.06 m.a. In two minutes after breaking the circuit the white

had changed to black, the extent of which had become 19 millim., *i. e.* nearly half the length of the film.

The change from white to black in an experiment such as this is very striking: the black does not begin at the top and spread downwards as might have been expected, but the whole area of the white passes by insensible gradations into black, and it is difficult to say at times whether it is black or white. This intermediate condition between black and white can generally be obtained by accelerating the thinning of a film by a downward current until white has been formed, and then breaking the circuit and leaving the film to itself.

This and similar observations show that a downward current has the remarkable effect of rapidly thinning that part of the film which is thicker than the black; but does not necessarily affect the latter. The film is, however, put into such a condition that, on the cessation of the current, the development of the black proceeds at a rapid rate.

(3) A film 40 millim. long had a ring of black 2.5 millim. in breadth at the top. A downward current of 6.2 m.a. was applied. In one minute the black entirely disappeared, being replaced by white. The current rose to 6.5 m.a.

The conditions in this case appear to be nearly the same as in the last. In each there are 2.5 millim. black, and the strength of current applied is about the same. Nevertheless in the latter the effect is to destroy the black and increase the conductivity of the film.

This is to be explained as follows:—When a film is formed between metal supports, the thin portion of the film is not immediately in contact with the metal. There is between them a thick ring of liquid of variable length, and from this the film proper hangs. As the film thins this massive ring of liquid is maintained more or less unaltered. Experiments were made with supports of various materials, shapes, and thicknesses, with the object of determining whether these had any marked effect on the mode or rate of thinning of the films. Glass, iron, and platinum have been used, and the edges of the supports made thick and rounded or finely bevelled. Although a thick and rounded edge would support a larger quantity of liquid than a sharp edge, the thinning of the films seemed little influenced by these modifications. When therefore a downward current is passed through a film having a certain area of black immediately depending from a ring of thick liquid, the current, carrying matter with it, soon floods the black with liquid and causes it to appear first grey and then white, its thickness and therefore its conductivity increasing. In the same way may be explained the fact that when a down-

ward current flows through a film from the moment of its formation, although the film thins more or less rapidly to the white of the first order, it generally does not show the black so soon as it would have done if no current had been flowing. So long as a thick liquid fringe remains on the upper support, liquid is being carried downwards and the film prevented from thinning beyond a certain limit. As soon as the circuit is broken this downward passage of liquid ceases, and the white of the first order rapidly thins to black. The fact that the current sometimes does and sometimes does not destroy the black may be understood by reflecting that the increase or decrease in the thickness of any part of the film depends upon whether the quantity of matter carried into it exceeds or falls short of that transported away from it in the same time. Since both these effects are produced in part by gravity and in part by the action of the current, and are also probably modified by the thickness of the different parts of the film, it is possible that very slight differences in the conditions of the experiment may determine which of the two shall obtain the predominance.

(4) Film 30 millim. long. A downward current of 24 m.a. applied from the beginning. In ten minutes the white extended more than halfway down, and the current fell to 2.5 m.a. The circuit was then broken. The white turned to black so rapidly that in less than one minute the entire transformation was effected.

In this case, the film being shorter, the same battery gave a stronger current, and the changes due to the current were accelerated.

We will now describe a few typical cases of the action of an upward current.

ii. *Upward Current. Plain Potash-soap Solution.*

(1) Film 40 millim. long. Current (9.2 m.a.) applied from the beginning. In twenty-four minutes there was a narrow band of white at the top succeeded by a narrow band of indigo-blue. Faint uniform tint below. The film was left for three quarters of an hour, but no black appeared. The current fell to 1.95 m.a. We have evidence here of the retarding action of the current, as the film would have exhibited black in ten or twelve minutes had there been no current. The current was too feeble to produce more marked changes.

(2) Film 40 millim. long. There were 3 millim. of black and three millim. of white immediately below. An upward current of 4.9 m.a. was used. The lower edge of the black

became blurred and indistinct, and in two minutes the black had disappeared, white taking its place. The current rose to 5.32 m.a. The circuit was then broken. Three millim. of black having been formed in seven minutes, the current was again put on ($C=4.2$ m.a.). The fuzziness at the edge reappeared at once, and in two minutes the black was all gone. The current rose to 4.62 m.a.

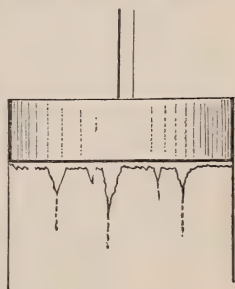
(3) Film 19 millim. long, entirely black from top to bottom. Upward current of 3.12 m.a. put on. In thirty-eight minutes the current rose to 4.23 m.a., and the film was white throughout.

(4) Film 30 millim. long. Black appeared in ten minutes. When it had increased to 5 millim. the circuit was completed. $C = 5.05$ m.a. In one minute all the black had disappeared, and in two minutes more the film had a uniform tint throughout, while the current rose to 14.5 m.a.

(5) Film 30 millim. long. Upward current passed through from the beginning. It was not measured at first, but after five minutes its strength was 20 m.a. This increased to 28.5 m.a., which was maintained for several minutes, after which it slowly fell. The film was colourless.

The ring of liquid on the upper platinum support was seen to increase in thickness and to become wavy along its lower edge. Pendent drops were formed, pointed at the bottom,

and hanging from the liquid fringe. From the points of the drops tiny streamlets were seen flowing down the film. This is a very remarkable experiment. The up-current not only retards the thinning, but actually increases the thickness of the film. The strength of the current was not observed at first; but from the moment when it was observed until it reached its maximum it increased by over 40 per cent., and the thickness of the film must



have increased in nearly the same proportion. In opposition to gravity the current carries liquid to the top of the film, where it accumulates and from which it descends in liquid veins.

The phenomena mentioned above recurred in several other films similarly treated. In some cases the film lasted long enough after the circuit was broken for black to be formed and to extend to a considerable depth; and the curious spectacle was exhibited of pendent drops of thick colourless liquid penetrating through the black to a distance varying from $\frac{1}{2}$ millim. to $9\frac{1}{2}$ millim.

(6) Film 30 millim. long. When the black, which appeared in ten minutes, had increased to 14 millim. a current was passed. $C=1.9$ m.a. In five minutes the black was reduced to 8 millim., and then the film broke. This shows that even a feeble current is, at certain stages of a film's existence, sufficient to destroy the black.

(7) The last case we may mention was one in which two films, each 30 millim. long, were formed side by side. The current from the battery was divided and passed up one and down the other film. The effect was most striking. In twelve minutes, when one of the films broke, there were 15 millim. of black in the one, while the other was thick and colourless and liquid was streaming from the top of it.

The experiments cited above were made with a solution of potash-soap (Brit. Pharm.) without any glycerine. The advantage of using such a solution is that the films formed with it grow thin with tolerable rapidity, and in the course of two or three hours several films may be observed with each apparatus. Similar experiments were carried out with *liquide glycérique* (made both with oleate of soda and potash-soap). The results were identical in kind with those enumerated above, but very different in degree. Films made with this solution are very persistent, but thin slowly. Generally three or four hours elapsed before any black appeared; and when a current was passed through, its effect was not so promptly manifested as in the case of the plain soap solution. The addition of nitrate of potassium may be partly accountable for this sluggishness, for it certainly increases the viscosity of the liquid. Nevertheless a great number of films made of the glycerine solution were observed, with currents flowing up or down. The effect of the downward current was invariably to promote, of the upward current to retard, the thinning of the film.

XIII. On the Rotation of the Plane of Polarization of Light by Reflection from the Pole of a Magnet.

To the Editors of the *Philosophical Magazine and Journal*.

Trinity College, Dublin,
December 27, 1884.

GENTLEMEN,

PROF. A. KUNDT, in his paper translated from the Berlin *Sitzungsberichte* into the October number of the *Philosophical Magazine* does me the honour of noticing the theoretical explanation I have given (R. S. Proc. xxv. p. 447) of the rotation of the plane of polarization of light by reflection

from the pole of a magnet. From the way in which he writes of my theory being refuted by his experiments, it is plain that he is unacquainted with my paper "*On the Electromagnetic Theory of the Reflection and Refraction of Light*" (*R. S. Trans.* 1880, part ii. p. 691), in which the following sentences occur (p. 709):—

"In comparing these expressions with the results of Mr. Kerr's admirable experiments, it is necessary to observe, as I mentioned before, that the introduction of a difference of phase between the reflected components is a question of a different order from that here discussed, and probably to some extent at least depends on the want of abruptness in the change from one medium to the other. For instance, my expressions give no change of plane of polarization when light is reflected normally from the end of a magnet, but they would lead one to expect that the only effect was a slight elliptic polarization, the major axis of the ellipse being in the same plane as the original plane of polarization. Now Mr. Kerr's experiments show that there is some rotation of this plane by reflection, and a supposition similar to one long ago proposed to explain the known elliptic polarization of metallic reflection—namely, that the efficient reflecting surface has some depth—may easily be shown to lead to Mr. Kerr's result. On this hypothesis the reflected ray is the resultant of the rays reflected from a small thickness at the surface of separation of the media; and in the case of normal reflection from the end of the pole of a magnet, each of these components would be slightly turned from its original plane of polarization owing to having passed through a very small thickness of a very powerful rotatory polarizing substance—namely, this superficial layer of the magnet; hence it is evident that their resultant would no longer be polarized in the same plane as the incident ray. I only give this as an instance of how this question of a difference of phase affects the results, and how the hypotheses that have been framed to explain it might be used to bring my results into complete accord with Mr. Kerr's experiments."

Prof. Kundt will see from this that I have anticipated the objection he raises, and have also anticipated him in pointing out in what direction to look for the explanation of the outstanding phenomena, to one of which he has called attention. Even in my first paper, the one with which Prof. Kundt is acquainted, I pointed out that it would be necessary to include the complications of metallic reflection in order to arrive at a complete theory. I cannot agree with Prof. Kundt in thinking that the incompleteness he has now pointed out and

which I pointed out in 1880 is in any fair sense a refutation of my theory. Fresnel's theory of reflection cannot be fairly said to be refuted because it leaves unexplained the gradual change of phase of the components near the polarizing angle. All that is required in order to make my theory more complete is a little fearlessness as to complicated mathematics, which I regret to say either my laziness or my busyness prevents me from displaying, as it has till now prevented my noticing Prof. Kundt's paper.

I have roughly compared my formula with Prof. Kundt's results, and find that the calculated rotations are of the same order of magnitude as he has observed. As he does not give experiments on the rotations produced by reflection and transmission under exactly similar conditions, any comparison must of necessity be rough.

Yours obediently,

GEORGE FRANCIS FITZGERALD.

XIV. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part IX. By C. R. ALDER WRIGHT, D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School.

[Continued from p. 29.]

B. *Voltaic and Thermovoltaic Constants of Metals immersed in Solutions of their Chlorides.*

I. *Cadmium.*

180. A number of cells were set up with amalgamated-zinc and electro-cadmium plates, and solutions of equal molecular strength, m MCl_2 100 H_2O , on each side, and examined precisely as before. The following average values were obtained, the probable error in each case being from $\pm \cdot 001$ to $\pm \cdot 002$ volt. The values of E_H annexed are calculated from the heat-valuations of Julius Thomsen, viz. Zn , Cl_2 aq. = 112840, and Cd , Cl_2 aq. = 96250, for molecular strength $\cdot 25$ MCl_2 100 H_2O ; so that the heat of displacement of cadmium by zinc from chloride solution m CdCl_2 100 H_2O is

$$112840 - h_1 - (96250 - h_2) = 16590 - (h_1 - h_2),$$

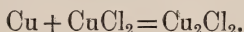
where h_1 and h_2 are the heats of dilution of zinc- and cadmium-chloride solutions respectively from the strength m MCl_2 100 H_2O to $\cdot 25$ MCl_2 100 H_2O : the values of h_1 and h_2 are derived from the table given in § 160, Part VIII.

Value of m .	E .	$16590 - (h_1 - h_2)$.	H .	$E - E_H$.
·25	·330	16590	·366	—·036
·5	·329	16190	·357	—·028
1·0	·324	15340	·338	—·014
2·0	·315	13890	·306	+·009
4·0	·307	12290	·271	+·036
6·0	·296	11390	·251	+·045
8·0	·262	10890	·240	+·022

It is here evident that whilst the voltaic constant for cadmium-zinc-chloride cells continually diminishes as the solution-strength increases, the value of E_H diminishes still more rapidly; so that the thermovoltaic constant is negative for weaker solutions and positive for stronger ones, the actual values of this constant being somewhat larger than those observed with zinc-cadmium-sulphate cells (§ 172), but still always under 5 centivolts. With the sulphate cells the values of the constant were opposite in sign, viz. positive for weaker, and negative for stronger solutions.

II. Copper.

181. On immersing a plate of clean metallic copper in a strong solution of cupric chloride, the plate speedily becomes visibly coated with a film of sparingly soluble cuprous chloride formed in virtue of the reaction



With weaker solution the actual deposition of cuprous chlorides in the solid state is not always noticeable, although its formation and presence are readily detected analytically. It is hence evident that any cell containing as one half a solution of cupric chloride and a copper plate must really contain as the electrolyte on this side a mixture of cuprous chloride with more or less cupric chloride, the film of fluid in immediate contact with the copper plate containing but little if any of the latter salt in solution. In point of fact, on examining a number of similar cells set up with electro-copper plates immersed in ·25 CuCl_2 100 H_2O on the one side and amalgamated-zinc plates immersed in ·25 ZnCl_2 100 H_2O on the other, values were obtained fluctuating between 1·083 and 1·106 volt; whilst another series of analogous values fluctuating between almost exactly the same limits, and averaging practically the same value, was obtained with a corresponding set of cells in which the copper plate was immersed, not in cupric-chloride solution alone to begin with, but in a magma

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of cuprous-chloride crystals suspended in $\cdot 25 \text{ CuCl}_2 \cdot 100 \text{ H}_2\text{O}^*$.
The values of the two series of observations combined were as
follows :—

Maximum E.M.F.	1·106
Minimum	„	1·083
Average	„	1·0985
Probable error of average value	$\pm \cdot 0018$

It is noticeable in this connection that precisely analogous results were obtained with other sets of cells, containing, on the one hand, cupric chloride alone, and on the other a magma of cuprous-chloride crystals and cupric chloride, when opposed to cadmium; whilst similarly various mercury cells gave substantially the same values whether pure metallic mercury and corrosive-sublimate solution were used, or mercury and a magma of calomel and corrosive-sublimate solution (§ 183).

It results from the above average value that the voltaic constant for electro-copper immersed in $\cdot 25 \text{ CuCl}_2 \cdot 100 \text{ H}_2\text{O}$ (or, what is the same thing, a magma of this solution and crystals of cuprous chloride) is $+1\cdot 0985$, when referred to amalgamated zinc immersed in $\cdot 25 \text{ ZnCl}_2 \cdot 100 \text{ H}_2\text{O}$ as zero. Sensibly the same value results as the sum of the E.M.F.'s of zinc-cadmium and cadmium-copper cells in solutions of strength $\cdot 25 \text{ MCl}_2 \cdot 100 \text{ H}_2\text{O}$ throughout. With the latter cells the following numbers were obtained :—

Maximum	·778
Minimum	·756
Average	·769
Probable error	$\pm \cdot 0018$

Voltaic constant from Zinc-copper cells $=1\cdot 0985$

Do. from Zinc-cadmium + Cadmium-
copper cells } $\cdot 330 + \cdot 769 = 1\cdot 099$

Mean $=1\cdot 099$

The heat of displacement of copper from cuprous chloride by zinc with formation of $\cdot 25 \text{ ZnCl}_2 \cdot 100 \text{ H}_2\text{O}$ results from Thomsen's values, thus :—

Zn, Cl_2 , aq.	112840
Cu ₂ , Cl_2	65750

$47090 = 1\cdot 038 \text{ volt.}$

* The cuprous chloride was prepared by boiling together copper-sulphate solution with common salt and spongy copper, filtering hot, and allowing cuprous chloride to crystallize out on cooling.

Whence the thermovoltaic constant for electro-copper in contact with cuprous chloride suspended in cupric-chloride solution of strength $\cdot 25 \text{ CuCl}_2$ 100 H_2O is $1\cdot099 - 1\cdot038 = +\cdot061$.

The experiments detailed in § 174 indicated that when lead-sulphate was suspended in cadmium-sulphate solution a higher E.M.F. was observed, *cæteris paribus*, with zinc-lead-sulphate cells than when zinc-sulphate solution was employed; from which it would seem probable that, if cuprous chloride be suspended in zinc-, cadmium-, and copper-chloride solutions in three zinc-copper-chloride cells otherwise alike, the E.M.F. will be smallest in the first case and greatest in the last. This is in fact the case. The following values were obtained in a number of experiments on the point, indicating that the use of cadmium-chloride solution gives values intermediate between those obtained with zinc- and copper-chloride solutions respectively. The solutions were in all cases of strength $\cdot 25 \text{ MCl}_2$ 100 H_2O .

	Cu_2Cl_2 suspended in zinc chloride.	Cu_2Cl_2 suspended in cadmium chloride.
Maximum E.M.F. ...	$\cdot 993$	$1\cdot011$
Minimum „ ...	$\cdot 980$	$\cdot 993$
Average „ ...	$\cdot 988$	$1\cdot001$
Probable error	$\pm \cdot 0028$	$\pm \cdot 0035$

Hence the thermovoltaic constants for electro-copper in contact with cuprous chloride suspended in zinc- or cadmium-chloride solutions of strength $\cdot 25 \text{ MCl}_2$ 100 H_2O are *negative* in sign, viz.:—

Suspended in zinc-chloride solution . $\cdot 988 - 1\cdot038 = -\cdot050$
 „ cadmium „ „ . $1\cdot001 - 1\cdot038 = -\cdot037$

It should hence result that cells set up with electro-cadmium immersed in $\cdot 25 \text{ CdCl}_2$ 100 H_2O on the one side, and on the other side electro-copper immersed in a magma of cuprous chloride suspended in zinc-chloride solution of the same strength, will have an E.M.F. of $\cdot 672 - \cdot 050 - (-\cdot 036) = \cdot 658$; and, similarly, that analogous cells set up with cuprous chloride suspended in cadmium-chloride solution will have an E.M.F. of $\cdot 672 - \cdot 037 - (-\cdot 036) = \cdot 671$ ($\cdot 672$ being the E.M.F. corresponding with the heat of displacement of copper

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 from cuprous chloride by cadmium = $96250 - 65750 = 30500$).
 On setting up a number of such cells the following values
 were obtained, the averages being sensibly identical with
 those thus calculated :—

	Cu_2Cl_2 suspended in zinc chloride.	Cu_2Cl_2 suspended in cadmium chloride.
Maximum E.M.F.....	·667	·674
Minimum „	·654	·666
Average „	·659	·671
Probable error	± 0027	± 0016

It is noticeable in connection with these values, that whereas zinc-cadmium, cadmium-copper, and zinc-copper cells set up with sulphate solutions of various strengths possess E.M.F's. in no case differing by more than ± 010 volt from the values calculated from the difference between the heats of formation of the two electrolytes, the divergence between the two values is usually much greater when corresponding cells are set up with chloride solutions. Thus, for example :—

	Calculated.	Observed.	Difference.
Zinc-Cadmium ($\cdot 25 \text{MCl}_2$ $100 \text{H}_2\text{O}$) ...	·366	·330	—·036
Do. ($6\cdot 0 \text{MCl}_2$ $100 \text{H}_2\text{O}$)	·251	·296	+·045
Zinc-Copper (Cu_2Cl_2 in ZnCl_2)	1·038	·988	—·050
Do. (Cu_2Cl_2 in CdCl_2)	1·038	1·001	—·037
Do. (Cu_2Cl_2 in CuCl_2)	1·038	1·099	+·061
Cadmium-Copper (Cu_2Cl_2 in ZnCl_2)...	·672	·659	—·013
Do. (Cu_2Cl_2 in CdCl_2) ...	·672	·671	—·001
Do. (Cu_2Cl_2 in CuCl_2)...	·672	·769	+·097

III. Silver.

182. A number of cells were set up with amalgamated-zinc and zinc-chloride solution on one side, and on the other side electro-silver immersed in a magma of freshly precipitated, well-washed silver chloride, suspended in zinc-chloride solution. The following values were finally obtained for cells where the zinc-chloride solution is of the same strength, $m\text{ZnCl}_2$ $100 \text{H}_2\text{O}$,

on each side. The probable error is in all cases below ± 0.003 .

The thermovoltaic constants for electro-silver thus immersed are calculated from the heat of displacement of silver from silver chloride by zinc forming $m\text{ZnCl}_2$ 100 H_2O , viz. :—

$$112840 - 58760 - h = 54080 - h;$$

where

$$112840 = \text{Zn, Cl}_2, \text{ aq. } (m = .25),$$

$$58760 = \text{Ag}_2, \text{ Cl}_2,$$

$$h = \text{heat of dilution of } m\text{ZnCl}_2 \text{ 100 H}_2\text{O to } .25 \text{ ZnCl}_2 \text{ 100 H}_2\text{O } (\S 160).$$

m .	h .	$54080 - h$.	E_H .	E .	$E - E_H$.
.25	0	54080	1.192	1.080	-.112
.5	400	53680	1.184	1.068	-.116
1.0	1250	52830	1.165	1.049	-.116
3.0	3800	50280	1.109	1.026	-.083
6.0	5300	48780	1.076	1.014	-.062

Hence it results that both the voltaic and thermovoltaic constants diminish in value as the solutions become stronger; and that whilst the thermovoltaic value of silver in contact with silver chloride (suspended in zinc-chloride solution) is negative in sign as with the sulphate, its numerical value is considerably less than in that instance.

Some cells were next examined, containing on the one side electro-silver plates immersed in a magma of silver chloride suspended in zinc-chloride solution, and on the other side electro-copper or electro-cadmium in their respective chloride solutions, the solution-strength being $.25\text{MCl}_2$ 100 H_2O throughout. The following values were obtained, sensibly agreeing with the value $-.112$ for the thermovoltaic constant above obtained with zinc-silver-chloride cells for this molecular strength. It is to be noticed that the values of E_H with these two kinds of cells respectively are $.827$ and $.154$, corresponding with the heat-evolutions $96250 - 58760 = 37490$, and $65750 - 58760 = 6990$. In the former case $E_H + (k_1 - k_2)$ is positive in sign, in the latter case negative; so that the copper-silver cells are analogous to the zinc-cadmium-sulphate and

cadmium-iron-sulphate cells above described, in that the metal actually acquiring the higher potential is not the one predicable from the formation-heats of the electrolytes, but the other one—a result indicated by the — sign prefixed to the values of E.

	Cadmium-Silver.	Copper-Silver.
Maximum E.M.F. observed.....	·758	— ·017
Minimum „ „	·743	— ·025
Average „ „	·751	— ·020
Probable error	± ·0017	± ·0015
Value of thermovoltaic constant deduced from average observed E.M.F.	+ ·751	— ·020
	+ ·330	+ 1·099
	—	—
	+ 1·081	+ 1·079
	— 1·192	— 1·192
	— — — — — ·111	— — — — — ·113

The general average of the three values — ·112, — ·111, and — ·113 thus obtained with zinc-silver, cadmium-silver, and copper-silver cells respectively is — ·112, representing the thermovoltaic constant for electro-silver in contact with precipitated silver chloride suspended in zinc-chloride solution of strength ·25 ZnCl₂ 100 H₂O.

In precisely similar fashion six other kinds of cells were examined, containing respectively zinc in zinc-chloride, cadmium in cadmium-chloride, and copper in cupric-chloride solutions, opposed in one series to silver immersed in silver chloride suspended in cadmium-chloride solution, and in the other to silver immersed in silver chloride suspended in cupric-chloride solution, the solution-strength being ·25 MCl₂ 100 H₂O throughout. In the copper-silver cells, where the silver chloride was suspended in cadmium-chloride solution, the same peculiarity is observable as when suspended in zinc-chloride solution, viz. that the copper, and not the silver, acquires the higher potential; but where the silver chloride is suspended in cupric-chloride solution this is not the case, the silver here acquiring the higher potential, and the current flowing in the direction predicable from the heats of formation of the electrolytes, and not in the opposite direction. The following values were obtained :—

Silver Chloride suspended in Cadmium-Chloride Solution.			
	Zinc-Silver.	Cadmium-Silver.	Copper-Silver.
Maximum	1·091	·766	—·009
Minimum	1·082	·757	—·014
Average	1·088	·761	—·010
Probable error	± ·0010	±·0010	±·0012

Silver Chloride suspended in Cupric-Chloride Solution.			
Maximum	1·139	·809	+·038
Minimum	1·128	·806	+·031
Average	1·136	·807	+·035
Probable error	± ·0017	±·0006	±·0015

From these figures the following concordant values result for the thermovoltaic constants of electro-silver immersed in silver chloride suspended in cadmium and copper chlorides respectively :—

Silver Chloride suspended in Cadmium-Chloride Solution.			
	E.	E _H .	E—E _H .
Zinc-Silver cells.....	1·088	1·192	—·104
Cadmium-Silver + Zinc-Cadmium {	+ ·761	1·192	—·101
	+ ·330		
Copper-Silver + Zinc-Copper ... {	+ 1·099		
	— ·010	1·192	—·103
	Average 1·089		—·103

Silver Chloride suspended in Cupric-Chloride Solution.			
Zinc-Silver cells.....	1·139	1·192	—·053
Cadmium-Silver + Zinc-Cadmium {	+ ·807	1·192	—·055
	+ ·330		
Copper-Silver + Zinc-Copper..... {	+ 1·099		
	+ ·035	1·192	—·058
	Average 1·137		—·055

It is noticeable that suspending silver chloride in zinc-, cadmium-, or copper-chloride solution affects the E.M.F. of a cell in the same way as the use of the same fluids for the suspension of cuprous chloride (§ 181), viz. that the E.M.F. is lowest with zinc and highest with copper-chloride solution.

Since the E.M.F. of a zinc-silver-chloride cell continually diminishes as the solution-strength rises, just as with a Clark's cell (§ 179), it is evident that De-la-Rue and Müller's cell must possess an E.M.F. variable with the strength and nature of the solution used. A number of rods of pure silver chloride fused round strips of pure silver were obtained from Messrs. Johnson and Matthey, and set up opposed to zinc and cadmium in various solutions of their chlorides respectively. Variations of 4 to 5 centivolts were observed in the E.M.F. of such cells with different silver-chloride rods, everything else being the same, and the observations being made with the electrometer to avoid possible diminution in E.M.F. The average values found were slightly lower than those found with the same solution-strengths and precipitated silver chloride, as might perhaps be expected, inasmuch as the silver strips inside the fused rods were not electro-coated but were of bright metal, which has been found in the case of sulphate-cells to give lower values than electro-coated metal (§ 125).

	Zinc-Silver.		Cadmium-Silver.	
	$\cdot 25 \text{ Zn Cl}_2$ 100 H_2O .	$2\cdot 0 \text{ Zn Cl}_2$ 100 H_2O .	$\cdot 25 \text{ Cd Cl}_2$ 100 H_2O .	$2\cdot 0 \text{ Cd Cl}_2$ 100 H_2O .
Maximum	1·097	1·048	·776	·740
Minimum	1·058	·995	·726	·714
Average	1·075	1·020	·750	·730
Value with precipitated } silver chloride	1·080	1·037	·761	...

IV. Mercury.

183. When metallic mercury and corrosive-sublimate solution are brought into contact, calomel is formed; so that by agitating the two together almost the whole of the metal and chlorine are removed from solution. Hence it is to be expected that the same peculiarity would apply to these cells as to copper cells (§ 181), viz. that much the same E.M.F. will be given whether mercury be in contact with corrosive-sublimate solution, or with a magna of calomel and corrosive-sublimate solution. In point of fact this is the case; cells set

up in the two ways with the mercury opposed to cadmium (or zinc) giving values as nearly identical as the considerably wide range of fluctuation in E.M.F. observed with different cells would permit to be deduced. In each case the values ranged between ·89 and ·95 volt (cadmium), and between 1·21 and 1·28 (zinc). Combining the two sets of cells, the following values were obtained, the solution being ·25 MCl₂ 100 H₂O throughout.

	Zinc-Mercury.	Cadmium-Mercury.
Maximum	1·282	·948
Minimum	1·207	·892
Average	1·256	·929
Probable error ...	± ·0056	± ·0053

From these two values nearly the same voltaic constant is deduced for mercury in contact with mercuric-chloride solution (or with a magma of mercuric chloride suspended in mercuric-chloride solution) of strength ·25 HgCl₂ 100 H₂O.

Zinc-mercury 1·256

Cadmium-mercury + Zinc-cadmium . $\left\{ \begin{array}{l} \cdot 929 \\ \cdot 330 \end{array} \right\} = 1·259$

Mean 1·257

Since Hg₂Cl₂=82550, the value of E_H corresponds with 112840—82550=30290 gramme-degrees for zinc-mercury-chloride cells, and with 96250—82550=13700 for cadmium-mercury-chloride cells, or ·668 and ·302 volt respectively; whence evidently both classes of cells resemble lead-copper-sulphate cells (§ 174) in actually giving rise to an E.M.F. greatly superior to that corresponding with the net chemical change. The thermovoltaic constant deduced from the above mean voltaic constant is 1·257—·668=+·589; so that mercury in this class of cells resembles iron, aluminium, and magnesium in their sulphate cells, possessing a large positive thermovoltaic constant.

Some observations were next made with cells containing calomel suspended in zinc-chloride solution in contact with mercury on one side, and amalgamated zinc immersed in the same zinc-chloride solution on the other side. Like the analogous zinc-silver-chloride, zinc-lead-sulphate, and zinc-mercury-sulphate cells above described, the E.M.F. of this combination was found to decrease as the solution-strength

112 Messrs. Wright and Thompson on the Determination of rises. Thus the following numbers were obtained with solution-strengths $m\text{ZnCl}_2$ 100 H_2O , the probable error being from $\pm \cdot 002$ to $\pm \cdot 003$ in each case.

$m = \cdot 25$	1.123
$= 1.0$	1.093
$= 5.0$	1.043
$= 9.5$.988

From these values the following thermovoltaic constants result, the values of E_H being calculated from the heat-values $112840 - 82550 - h = 30290 - h$, where h is heat of dilution of $m\text{ZnCl}_2$ 100 H_2O to $\cdot 25 \text{ZnCl}_2$ 100 H_2O .

m .	h .	$30290 - h$.	E_H .	E .	$E - E_H$.
$\cdot 25$	0	30290	.668	1.123	+ .455
1.0	1250	29040	.640	1.093	+ .453
5.0	4800	25490	.562	1.043	+ .481
9.5	6000	24290	.536	.988	+ .452

Observations with cells containing electro-cadmium immersed in $\cdot 25\text{CdCl}_2$ 100 H_2O opposed to mercury in contact with calomel suspended in zinc-chloride solution of the same strength led to nearly the same value for the thermovoltaic constant when $m = \cdot 25$:—

Maximum E.M.F. observed806
Minimum " "	.791
Average " "	.799
Probable error	$\pm \cdot 0030$

	Voltaic constant.	$E - E_H$.
Zinc-cadmium + Cadmium-mercury... $\left\{ \begin{smallmatrix} .799 \\ .330 \end{smallmatrix} \right\}$	$= 1.129$	+ .461
Zinc-mercury cells.....	$= 1.123$	+ .455
Mean	1.126	+ .458

Substituting cadmium-chloride solution for zinc-chloride solution as the menstruum in which the calomel is suspended raises the value of E , as might be anticipated from various analogous results above described. Thus the following values were obtained with zinc-mercurous-chloride and cadmium-mercurous-chloride cells so set up, the amalgamated-zinc plates being surrounded by zinc-chloride, and the electro-cadmium plates employed with cadmium-chloride solution, the solution-strength being uniformly $\cdot 25 \text{MCl}_2$ 100 H_2O .

	Zinc-Mercury.	Cadmium-Mercury.
Maximum E.M.F. observed.....	1·144	·815
Minimum „ „	1·137	·808
Average „ „	1·140	·812
Probable error	\pm ·0013	\pm ·0014

From these figures the following values result as the thermovoltic constant for mercury in contact with calomel suspended in $\cdot 25 \text{ CdCl}_2$ 100 H O_2 :—

	E.	E_H .	$E - E_H$.
Zinc-mercury	1·140	·668	+ ·472
Cadmium-mercury . $\left\{ \begin{array}{l} \cdot 812 \\ \cdot 330 \end{array} \right\}$	1·142	·668	+ ·474
Mean	1·141		+ ·473

From the values of the heats of formation of silver chloride and mercurous chloride respectively, it might, *à priori*, be expected that an E.M.F. of + ·525 should result (corresponding with $82550 - 58760 = 23790$ gramme-degrees) in silver-mercury-chloride cells, silver acquiring the higher potential. On the other hand, whether the silver chloride be suspended in zinc or cadmium-chloride solutions, and whether the mercurous chloride be similarly suspended, in all four cases the value of $k_1 - k_2$ is not only negative, but is numerically greater than $E_H = \cdot 525$; so that, in fine, the current flows in the opposite direction to that *à priori* predicable, the mercury acquiring the higher potential. Then the E.M.F. of the four kinds of silver-mercury-chloride cells are as follows :—

Calculated from formula $E = E_H + k_1 - k_2$.			Observed.		
AgCl suspended	Hg ₂ Cl ₂ suspended		Maximum.	Minimum.	Average.
in ZnCl ₂ ...	in ZnCl ₂ ...	$\cdot 525 + (-\cdot 112) - \cdot 458 = -\cdot 045$	— ·040	— ·051	— ·046
Ditto ...	in CdCl ₂ ...	$\cdot 525 + (-\cdot 112) - \cdot 473 = -\cdot 060$	— ·055	— ·062	— ·058
in CdCl ₂ ...	in ZnCl ₂ ...	$\cdot 525 + (-\cdot 103) - \cdot 458 = -\cdot 036$	— ·034	— ·041	— ·038
Ditto ...	in CdCl ₂ ...	$\cdot 525 + (-\cdot 103) - \cdot 473 = -\cdot 051$	— ·047	— ·053	— ·050

Considerably larger negative values were obtained with analogous cells in which mercurous chloride was suspended in mercuric-chloride solution instead of zinc or cadmium-chloride solution, as would naturally be inferred from the larger values of the thermovoltic constant in this case. The

forces considerably *below* the value of E_H pertaining to that class of cell, lead-silver-chloride cells possess electromotive forces considerably *above* the corresponding value of E_H .

On substituting lead-chloride solution for zinc-chloride solution as the medium in which silver chloride is suspended, but little change is produced in the E.M.F. Thus a number of lead-silver-chloride cells containing solution of strength $\cdot 25 \text{ PbCl}_2$ 100 H_2O throughout gave values ranging from $\cdot 453$ to $\cdot 522$ volt, and averaging near to $\cdot 480$.

Some lead-copper-chloride cells were examined; but for some unknown reason they failed to give concordant results, the observed E.M.F. always falling short of that deduced from the voltaic constants for lead and copper in chloride-solution (viz. $1\cdot 099 - \cdot 591 = \cdot 508$). In some cases the falling off was small; in others it exceeded $\cdot 050$: these cells, moreover, exhibited no constancy even for half an hour after setting up, the E.M.F. rapidly falling as time elapsed. In all cases, however, higher electromotive forces were observed than correspond with the difference between $\text{Pb, Cl}_2 \text{ aq.} = 75970$ and $\text{Cu}_2, \text{Cl}_2 = 65750$, viz. 10220 gramme-degree $= \cdot 225$ volt.

On the other hand, numbers closely agreeing with those calculated from the above-described thermovoltaic constants were obtained with cells set up with electro-lead immersed in lead-chloride opposed to mercury in contact with mercurous chloride suspended in zinc- or cadmium-chloride solution, the strength being $\cdot 25 \text{ MCl}_2$ 100 H_2O throughout. The value of E_H in these cells is $+ \cdot 145$ volt, corresponding with $82550 - 75970 = 6580$ gramme-degrees, the metal giving the smaller heat-value, and consequently acquiring the higher potential in consequence of the chemical action, being lead. Since, however, the thermovoltaic constant for lead is $-\cdot 222$, and that for mercury in contact with mercurous chloride suspended in zinc-chloride solution $+ \cdot 458$, and when suspended in cadmium-chloride solution $+ \cdot 473$, the value of $k_1 - k_2$ is in each case largely negative, and considerably greater numerically than E_H . Hence the calculated electromotive forces for the two cells are respectively $\cdot 145 + (-\cdot 222) - \cdot 458 = -\cdot 535$, and $\cdot 145 + (-\cdot 222) - \cdot 473 = -\cdot 550$ volt. The observed values were $-\cdot 539$ and $-\cdot 549$ respectively, *i. e.* electromotive forces of $\cdot 539$ and $\cdot 550$ volt were observed, the mercury, and not the lead, acquiring the higher potential. These cells are remarkable not only for the large negative values of $k_1 - k_2$, but also for the high negative values of $E_H + k_1 - k_2$, exceeding in this respect all other cells examined with the exception of zinc-aluminium-sulphate cells (§ 178), which gave the numerical value $\cdot 537$.

VI. *Magnesium.*

185. A series of cells was set up containing bright magnesium (wire) opposed to amalgamated zinc and electro-cadmium in their respective chloride-solutions, the strength being uniformly $\cdot 25 \text{ MCl}_2$ $100 \text{ H}_2 \text{ O}$. The values exhibited much greater fluctuations as time elapsed than were observed with most of the other cells examined; whilst the mean readings during the first half-hour after setting up also showed much less concordance, as the following figures show. As with the sulphate-cells (§ 177), magnesium acquired the lower potential when opposed to zinc, and *à fortiori* when opposed to cadmium.

	Magnesium-Zinc.	Magnesium-Cadmium.
Maximum	$\cdot 771$	$1\cdot 101$
Minimum	$\cdot 636$	$\cdot 966$
Average	$\cdot 702$	$1\cdot 030$
Probable error ...	$\pm \cdot 012$	$\pm \cdot 012$

Hence the following valuations of the voltaic constant result, these values necessarily being negative, as with the sulphate-cells:—

$$\begin{array}{rcl}
 \text{Zinc-magnesium cell} & . & -\cdot 702 \\
 \text{Cadmium-magnesium} - \text{Zinc-cadmium} & \left\{ \begin{array}{l} -1\cdot 030 \\ + \cdot 330 \end{array} \right\} & -\cdot 700 \\
 \text{Mean} & . & -\cdot 701
 \end{array}$$

Julius Thomsen finds $\text{Mg, Cl}_2 \text{ aq.} = 186930$ for solution of strength $\cdot 25 \text{ MgCl}_2$ $100 \text{ H}_2 \text{ O}$; whence $E_H = -1\cdot 634$. Consequently the thermovoltic constant for bright magnesium in contact with chloride solution of this strength is

$$-\cdot 701 - (-1\cdot 634) = +\cdot 933,$$

or, approximately, the same large positive quantity as that found with sulphate solutions.

VII. *Aluminium.*

186. In precisely the same way were two series of observations made with cells containing plates of bright aluminium opposed to amalgamated zinc and electro-cadmium immersed in solutions of their chlorides respectively, the solution-strength being $\cdot 25 \text{ Al}_3 \text{ Cl}_2$ $100 \text{ H}_2 \text{ O}^*$, and equivalent amounts

* The aluminium-chloride solution was prepared by saturating dilute hydrochloric acid with recently precipitated well-washed aluminium hydroxide.

throughout. As with the magnesium cells, the values fluctuated considerably, so that the final averages had much larger probable errors than usual.

	Zinc-Aluminium.	Aluminium-Cadmium.
Maximum	-.239	+.091
Minimum	-.352	-.022
Average	-.280	+.050
Probable error	±.011	±.011

Each of these averages leads to the same value, +.280, for this voltaic constant.

$$\begin{array}{rcl}
 \text{Zinc-aluminium} & . & +.280 \\
 \text{Zinc-cadmium} - \text{Aluminium-cadmium} & \left\{ \begin{array}{l} +.330 \\ -.050 \end{array} \right\} & +.280 \\
 \text{Mean} & . & +.280
 \end{array}$$

This constant being of + sign, since aluminium was found to acquire the higher and zinc the lower potential (as with the sulphate-cells, § 178); whilst, on the other hand, the readings observed with the zinc-aluminium cells are marked - above because this cell is another example of the case where $E_H + k_1 - k_2$ has a negative value, the current actually set up passing in the direction opposite to that predicable from the heats of formation of the electrolytes.

Julius Thomsen finds $\text{Al}_3, \text{Cl}_2 \text{ aq.} = 158550$, whence $E_H = -1.008$; consequently

$$E - E_H = +.280 - (-1.008) = +1.288;$$

i. e. the thermovoltaic constant for bright aluminium in contact with its chloride solution is a large positive quantity, as with the sulphate.

VIII. Iron.

187. Cells were set up containing plates of nearly pure sheet-iron (bright) opposed to amalgamated zinc and electro-cadmium in solution of constant strength .25 MCl_2 100 H_2O ; also to electro-silver immersed in a magma of precipitated silver-chloride suspended in zinc-chloride solution of this same strength. The following numbers were obtained as the average readings during the first half-hour after setting up.

	Zinc-Iron.	Cadmium-Iron.	Silver-Iron.
Maximum	·497	—·154	·597
Minimum	·483	—·167	·583
Average	·490	—·157	·592
Probable error ...	± 0030	± 0027	± 0033

Hence the following three valuations for the voltaic constant result; the values with the cadmium-iron cell being marked —, since in this case (as with the corresponding sulphate cells, § 175) iron, and not cadmium, actually acquires the higher potential.

Zinc-iron ·490

Zinc-cadmium—Cadmium-iron . $\left\{ \begin{array}{l} \cdot 330 \\ + \cdot 157 \end{array} \right\} = \cdot 487$

Zinc-silver—Iron-silver . . . $\left\{ \begin{array}{l} 1\cdot 080 \\ - \cdot 592 \end{array} \right\} = \cdot 488$

Mean . . . $\cdot 488$

Julius Thomsen finds $\text{Fe}, \text{Cl}_2 \text{ aq.} = 99950$ for $\cdot 25\text{FeCl}_2$, $100\text{H}_2\text{O}$; whence $E_H = \cdot 284$. Hence the thermovoltaic constant for bright metallic iron in ferrous-chloride solution of this strength is

$$E - E_H = \cdot 488 - \cdot 284 = + \cdot 204;$$

i. e. it is a considerable *positive* quantity, as in the case of the sulphate-cells.

It is noticeable that whilst with silver-iron cells the value of $k_1 - k_2$ is negative, so that E is considerably less than E_H , the opposite holds with zinc-iron cells, the $E.M.F.$ actually set up being not much below double that corresponding with the net heat-development in the cell.

C. Voltaic and Thermovoltaic Constants of Metals immersed in Solutions of their Nitrates.

I. Copper.

188. A number of cells were set up with amalgamated-zinc and electro-copper plates, and solutions of the nitrates of the respective metals prepared by dissolving pure metals in nitric acid, and saturating any excess of acid by means of the metallic carbonate freshly precipitated from a portion of the respective solution, and well washed. The following values were obtained as the mean readings during the first twenty

minutes after setting up. The readings during this period did not always exhibit constancy, especially with the weaker solutions; but the extreme alteration usually only amounted to a few millivolts, concurrently with a visible alteration in the surfaces of the plates. Strength of solutions, $mM(NO_3)_2$ 100 H_2O .

	$m=.25$.	$m=1.0$.	$m=2.0$.	$m=8.0$.
Maximum	1.087	1.104	1.112	1.095
Minimum	1.053	1.073	1.108	1.087
Average	1.066	1.089	1.109	1.091
Probable error ...	$\pm .0035$	$\pm .0034$	$\pm .0005$	$\pm .0009$

These figures correspond with the following thermovoltaic constants, the values of E_H being deduced from the experiments of Julius Thomsen, including his valuations of the heats of dilution of zinc- and copper-nitrate solutions. The difference between the heats of formation of $mM(NO_3)_2$ 100 H_2O are given by the formula

$$102510 - h_1 - (52410 - h_2) = 50100 - (h_1 - h_2),$$

where Zn, O, N_2O_5 aq. = 102510, and Cu, O, N_2O_5 aq. = 52410 for solution-strengths .25 $M(NO_3)_2$, 100 H_2O ; whilst h_1 and h_2 are the heats of dilution of solutions of zinc and copper nitrate respectively from the strength $mM(NO_3)_2$ 100 H_2O to .25 $M(NO_3)_2$ 100 H_2O .

m .	h_1 .	h_2 .	$50100 - (h_1 - h_2)$.	E_H .	E .	$E - E_H$.
.25	0	0	50100	1.105	1.066	-.039
1.0	- 40	- 47	50093	1.105	1.089	-.016
2.0	-132	-175	50057	1.104	1.109	+.005
8.0	+439	+214	49875	1.100	1.091	-.009

It is here noticeable that the thermovoltaic constant for electro-copper in nitrate solution has in no case any large value; whilst of negative sign for low-solution strengths, it becomes positive for stronger fluids, and again negative with highly concentrated solutions, following the variations in the voltaic constant, which has a maximum value for fluids of medium strength.

II. Cadmium.

189. Cells were set up with electro-cadmium plates immersed in cadmium-nitrate solution, opposed on the one hand to electro-copper, and on the other to amalgamated zinc immersed in solutions of their nitrates respectively, the constant solution-strength being $\cdot 25 \text{ M}(\text{NO}_3)_2$ 100 H_2O . The following values were obtained :—

	Zinc-Cadmium.	Cadmium-Copper.
Maximum	$\cdot 359$	$\cdot 722$
Minimum	$\cdot 344$	$\cdot 707$
Average	$\cdot 351$	$\cdot 713$
Probable error ...	$\pm \cdot 0026$	$\pm \cdot 0024$

From these figures the following valuations result for the voltaic constant of cadmium in nitrate solution $\cdot 25 \text{ Cd}(\text{NO}_3)_2$ 100 H_2O :—

$$\begin{array}{rcl}
 \text{Zinc-cadmium} & . & \cdot 351 \\
 \text{Zinc-copper} - \text{Cadmium-copper} & . & 1\cdot 066 - \cdot 713 = \cdot 353 \\
 \text{Mean} & . & = \cdot 352
 \end{array}$$

Julius Thomsen finds Cd , O , N_2O_5 aq. = 86300 (for $\cdot 25 \text{ Cd}(\text{NO}_3)_2$ 100 H_2O), whence $E_{\text{H}} = \cdot 357$. Hence the thermovoltic constant for electro-cadmium in nitrate solution of this strength is

$$E - E_{\text{H}} = \cdot 352 - \cdot 357 = -\cdot 005.$$

It is here noticeable that the value of the thermovoltic constant is negative, as in the case of chloride solution of corresponding strength, whereas it is positive in the case of sulphate solution of the same strength; but in no case is the numerical value of the constant large.

III. Lead.

190. Cells containing electro-lead opposed to amalgamated zinc and electro-copper in nitrate solution of strength $m \text{ M}(\text{NO}_3)_2$ 100 H_2O gave the following average values, the probable error in no case exceeding $\pm \cdot 004$, and being usually considerably less :—

	$m = \cdot 25$.	$m = 1\cdot 0$.	$m = 2\cdot 0$.
Zinc-Lead	$\cdot 580$	$\cdot 588$	$\cdot 591$
Lead-Copper ...	$\cdot 486$	$\cdot 501$	$\cdot 519$

These two sets of figures lead to practically identical valuations of the voltaic constant:—

	$m = \cdot 25.$	$m = 1\cdot 0.$	$m = 2\cdot 0.$
Zinc-Lead	$\cdot 580$	$\cdot 588$	$\cdot 591$
Zinc-Copper — Copper-Lead $\left\{ \begin{array}{l} 1\cdot 066 \\ \cdot 486 \end{array} \right\}$	$\cdot 580$	$\left\{ \begin{array}{l} 1\cdot 089 \\ \cdot 501 \end{array} \right\} \cdot 588$	$\left\{ \begin{array}{l} 1\cdot 109 \\ \cdot 590 \end{array} \right\} \cdot 590$
Mean	$\cdot 580$	$\cdot 588$	$\cdot 591$

Taking the valuations of Julius Thomsen, including his determinations of the heats of dilution of zinc- and lead-nitrate solution, h_1 and h_2 respectively, the following values result for E_H , the value of Pb, O, N_2O_5 aq. being 68070 for $\cdot 25$ $Pb(NO_3)_2$ 100 H_2O :—

$m.$	$h_1.$	$h_2.$	H.	$E_H.$	E.	$E - E_H.$
$\cdot 25$	0	0	34440	$\cdot 759$	$\cdot 580$	$-\cdot 179$
1·0	— 40	— 1274	33206	$\cdot 732$	$\cdot 588$	$-\cdot 144$
2·0	— 132	— 2092	32462	$\cdot 716$	$\cdot 591$	$-\cdot 125$

Hence the thermovoltaic constant for electro-lead in nitrate solution is always negative (as with sulphate and chloride); the numerical value, however, decreases with increasing solution-strength, whilst the opposite is the case with lead sulphate, whether suspended in zinc- or in cadmium-sulphate solution.

Lead-copper-nitrate cells resemble lead-copper-sulphate cells in that they give an E.M.F. notably above that calculable from the net heat-evolution taking place in the cell due to the chemical change. Similarly zinc-lead-nitrate cells resemble zinc-lead-sulphate cells in giving values below those thus calculable.

IV. Silver.

191. The following mean values were obtained with cells set up with electro-silver plates opposed to amalgamated zinc, electro-copper, and electro-lead in nitrate solutions of strength $mM(NO_3)_2$, 100 H_2O (the probable error ranged from $\pm \cdot 002$ to $\pm \cdot 004$ throughout):—

$m.$	Zinc-Silver.	Copper-Silver.	Lead-Silver.
$\cdot 25$	1·495	$\cdot 429$	$\cdot 914$
1·0	1·540	$\cdot 450$	$\cdot 951$
2·0	1·556	$\cdot 446$	$\cdot 965$

From these figures sensibly identical values result for the voltaic constant of electro-silver in nitrate solution:—

	$m = \cdot 25.$	$m = 1\cdot 0.$	$m = 2\cdot 0.$
Zinc-silver	1.495	1.540	1.556
Zinc-Copper + Copper-Silver	$\left\{ \begin{array}{l} 1\cdot 066 \\ \cdot 429 \end{array} \right.$ 1.495	$\left\{ \begin{array}{l} 1\cdot 089 \\ \cdot 450 \end{array} \right.$ 1.539	$\left\{ \begin{array}{l} 1\cdot 109 \\ \cdot 446 \end{array} \right.$ 1.555
Zinc-Lead + Lead-Silver	$\left\{ \begin{array}{l} \cdot 580 \\ \cdot 914 \end{array} \right.$ 1.494	$\left\{ \begin{array}{l} \cdot 588 \\ \cdot 951 \end{array} \right.$ 1.539	$\left\{ \begin{array}{l} \cdot 591 \\ \cdot 965 \end{array} \right.$ 1.556
Mean ...	1.495	1.539	1.556

The heats of dilution of silver-nitrate solutions being as yet undetermined, the value of E_H can only be exactly calculated for the strength $\cdot 25 \text{ Ag}_2(\text{NO}_3)_2$ 100 H_2O . The subjoined values for the higher strengths are calculated on the supposition that the heat of dilution of silver- and zinc-nitrate solutions is the same; so that E_H remains the same for all solution-strengths Ag_2 , O, N_2O_5 aq. = 16780.

$m.$	$E.$	$E_H.$	$E - E_H.$
$\cdot 25$	1.495	1.890	— .395
1.0	1.539	1.890	— .351
2.0	1.556	1.890	— .334

Hence the thermovoltaic constant for silver in nitrate solution is not widely different from that in sulphate solution, being in each case far greater than that for silver in contact with silver chloride, whether suspended in zinc cadmium or cupric-chloride solution. With all three kinds of silver salt, however, under all circumstances, the value is negative in sign.

V. Mercury.

192. Mercuric-nitrate solution, like mercuric chloride, becomes rapidly and completely converted into mercurous salt by agitation with mercury. In order to prepare mercurous nitrate with but little excess of acid, mercury was dissolved in nitric acid, and the crystallized mass washed on the filter-pump with a little water, and then well agitated with water and mercury. The solution thus obtained was free from

nitrate solutions is a considerable + number, as found in the chloride-cells.

It hence follows that the E.M.F. actually set up in zinc-mercury, copper-mercury, and lead-mercury nitrate-cells is in excess of that corresponding with the difference between the heats of formation of the electrolytes in each case respectively, the excess amounts being $+ \cdot 297$, $+ \cdot 297 - (- \cdot 039) = + \cdot 336$, and $+ \cdot 298 - (- \cdot 179) = + \cdot 477$ respectively.

VI. Magnesium.

193. Two sets of cells were examined containing bright magnesium (wire), opposed to amalgamated zinc and electro-cadmium respectively, in solutions of constant strength $\cdot 25 \text{ M}(\text{NO}_3)_2$ $100 \text{ H}_2\text{O}$. The following values were obtained, the readings exhibiting the same kinds of fluctuations as those previously found with magnesium sulphate and chloride cells.

	Magnesium-Zinc.	Magnesium-Copper.
Maximum	$\cdot 570$	$1 \cdot 636$
Minimum	$\cdot 503$	$1 \cdot 569$
Average	$\cdot 531$	$1 \cdot 595$
Probable error	$\pm \cdot 011$	$\pm \cdot 011$

These two sets of readings lead to sensibly the same values for the voltaic constant, which is negative (as with the sulphate and chloride cells), since zinc now acquires the higher potential.

Zinc-magnesium $- \cdot 531$

Copper-magnesium . . . $\left\{ \begin{array}{l} -1 \cdot 595 \\ +1 \cdot 066 \end{array} \right\}$ $- \cdot 529$

Mean $- \cdot 530$

From Julius Thomsen's figures, the heat of formation of magnesium-nitrate solution, $\cdot 25 \text{ Mg}(\text{NO}_3)_2$ $100 \text{ H}_2\text{O}$, is 176480 , whence $E_H = -1 \cdot 631$. Hence the thermovoltaic constant is

$$- \cdot 530 - (-1 \cdot 630) = +1 \cdot 101,$$

or positive to a great extent, as with the other magnesium-cells.

[To be continued.]

XV. *Some Observations on the Behaviour of Electricity in Rarefied Air.* By Prof. E. EDLUND*.

FROM recent observations on the passage of electricity through rarefied gases, the conclusion has been drawn that the conductivity of a gas increases with its rarefaction until a certain limit has been reached ; but that if this limit is exceeded the conductivity begins to decrease, so that at last, if the process of exhaustion is continued, an absolute vacuum must be regarded as a nonconductor. These observations have been generally made by using an air-pump or other apparatus to rarefy the air in a glass vessel, into which two electrodes of platinum or some other metal had been melted, and then examining the passage of electricity through the rarefied gas. If we examine these experiments, which have been made at various times, and compare them critically with each other, we shall find that they by no means justify the conclusion hitherto drawn from them, viz. that the conductivity of gases attains its maximum for a certain definite rarefaction, and then diminishes again if the exhaustion is continued. On the contrary, everything goes to show that the conductivity increases continually up to the highest attainable limit, and that consequently an absolute vacuum is a good conductor. In fact the passage of electricity through a rarefied gas is not simply dependent upon the conductivity of the gas, but also to a considerable extent upon the greater or less ease with which the electricity itself effects its passage from the electrodes into the gas, or in the opposite direction. There is thus a resistance offered to the passage of electricity from the electrodes into the layers of gas in contact with them. To speak exactly, the experiments hitherto made show that this resistance increases with the exhaustion, whilst the resistance of the gas continually decreases. I have already endeavoured to show this in a previous research †.

The resistance which the electricity meets in its passage from the electrodes into the gas, or in the opposite direction, does not consist in an electrical resistance in the ordinary meaning of that term, but arises from an electromotive force, which acts in a direction opposed to that of the electric current. Direct observations, described in a former research ‡, showed that this opposed force increases con-

* Translated from a separate impression from Exner's *Repertorium der Physik*, communicated by the Author.

† Wied. *Ann.* vol. xv. p. 514 ; *Phil. Mag.* vol. xiii. p. 1.

‡ *Ann. de Chim. et de Phys.* [5] vol. xxviii. p. 114 ; *Phil. Mag.* vol. xv. p. 1.

tinuously in proportion as the exhaustion of the gas increases, and consequently opposes the passage of electricity by a resistance which increases with the exhaustion. The electromotive force in question is no other than that of which I showed the presence in the voltaic arc and in the electric spark some years ago *.

Although the existence of this force was at first denied by some physicists, who had no opportunity of examining the evidence which I furnished, and yet pronounced upon its value, yet it has been universally admitted in recent times, without further evidence being required.

Since the solution of the question whether a vacuum is a conductor or not is of great importance, not only as a matter of theory, but also as rendering possible a correct explanation of many phenomena in cosmical physics, I propose to describe here certain new experiments, which show the correctness of the results which I obtained in the researches to which I have referred. Experiments of the kind which I propose to describe have no doubt been already made by other physicists; but since they have not been so made as to fully show their significance, it is not superfluous to consider once more their importance.

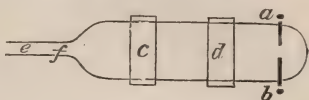
It is a well-known fact that the current of a Ruhmkorff's coil is not able to leap over even a small distance between two electrodes melted into a glass tube, if the air contained in the tube is sufficiently rarefied. If, however, a little more air is admitted into the tube, the current easily passes from the one electrode to the other. The problem, then, is as follows: Is the reason why the current will not pass through a highly-exhausted atmosphere that the resistance in the highly-exhausted gas is too great, or that the resistance opposed to the passage of the electricity from the electrodes into the atmosphere or in the opposite direction increases with the exhaustion?

We ought to obtain a clear and definite answer to this question, if we try, by means of an electromotive force, which is not greater than that of the Ruhmkorff's apparatus, to produce directly an electric current in the tube without electrodes. If we succeed thus in producing a current in the highly-exhausted atmosphere, this atmosphere must be a good conductor; and the reason that the current of the Ruhmkorff's apparatus will not pass through the tube must be sought in the resistance opposed by the electrodes themselves to the passage of the current.

First Experiment.—A glass tube, of the form shown in the

* *Bull. d. Acad. Suède*, 1867-68; *Pogg. Ann.* vols. cxxxi. & cxxxiv.; *Phil. Mag.* [4] vol. xxxvii. p. 352.

accompanying figure, 300 millim. long and of 16 millim. external diameter, is provided with two platinum wires, *a* and *b*, the ends of which were only 3 millim. apart, and attached by its small end, *f**e*, to the mercury-pump; *c* and *d* are two pieces of tinfoil which surround the tube without being in communication with the electrodes.



The current was produced by a large Ruhmkorff's induction-coil, actuated by two Bunsen-cells, the conducting-wires being connected alternately with the electrodes and with the tinfoil coatings. In the latter case, of course, the current of the induction-coil cannot penetrate into the glass; but the coatings at the moment of opening or closing the induction-current become charged, the one with positive, the other with negative electricity, and these charges disappear again equally rapidly. This continual charging and discharging of the coatings on the outside of the tube produce (as soon as the atmosphere of the tube is sufficiently exhausted) induced currents inside the tube, which show their presence by luminosity. This experiment, which, like all the rest, was made in a dark room, gave the following result:—

Pressure in tube.
millim.

531. Each interruption of the induced current produced a spark between *a* and *b*, but no discharge was observed in the tube between *c* and *d*.

355. The same result.

316. The same, except that the sparks between *a* and *b* are now more brilliant.

166. }
104. } There is still no discharge between *c* and *d*, but the
58. } sparks between *a* and *b* become very brilliant.
36. }

1. The luminosity between *c* and *d* becomes visible, and the spark between *a* and *b* becomes still more brilliant.

0·12. The luminosity between *c* and *d* is greater than before. The whole tube becomes luminous when the current passes from *a* to *b*.

0·017. Nearly the same result.

0·004. The current still passes from *a* to *b*, but with feeble light; intense luminosity between *c* and *d*.

0·00036. The spark between *a* and *b* is seen only occasionally, the tube remaining dark for long intervals of time. The induced light between *c* and *d*, on the other hand, is to be seen all the time, and is very intense.

If we consider first the phenomena attending the charge and discharge of the tinfoil coatings, we observe that when the density of the air in the tube is relatively considerable, *e. g.* 36 millim. or more, no perceptible induction-currents result; on the other hand, when the density amounts to less than 1 millim. down to 0.00036 millim., induction-currents are formed and manifest their presence by brilliant luminosity. The constancy of the induced electromotive force is altogether dependent upon the improvement which takes place in the conducting-power of the air in the same proportion as the rarefaction increases up to the limit attainable by means of the mercury air-pump. If, on the other hand, we observe the form assumed by the phenomenon when the current of the induction-coil is passed into the tube through the platinum electrodes, we find that the current easily leaps over the interval between the electrodes when the pressure amounts to one atmosphere or less down to 0.004 millim.—a point at which the resistance becomes large enough to sensibly weaken the current, and from which onward this resistance increases, if the exhaustion is continued; so that at a pressure of 0.00036 millim. the current can only occasionally leap over the interval between the electrodes, although this is only 3 millim. The resistance to the current at a pressure of only 0.00036 millim. is therefore greater than at a higher pressure. The two series therefore give altogether opposite results. I can only explain this by the presence of a hindrance to the passage of electricity from the electrodes into the air, whilst, on the contrary, the resistance peculiar to the air becomes less as its exhaustion increases. This experiment leads therefore to the same result as my former experiments, in which I came to the conclusion that there exists an opposing electromotive force at the surface bounding the electrodes and the gas, which increases continuously with the exhaustion of the gas.

Second Experiment.—The same tube was used, but the tinfoil coatings were made considerably broader than for the first experiment.

Pressure of air.
millim.

50. Visible sparks constantly between the electrodes *a* and *b*, but no induced light at all between the tinfoil coatings *c* and *d*.
3. Sparks between *a* and *b*. The induction-light between *c* and *d* begins to show.
- 0.05. The spark between *a* and *b* extends considerably towards the other end of the tube. Strong induction-light between *c* and *d*.

Pressure of air.
millim.

- 0·011. The spark between the electrodes *a* and *b* becomes smaller and less extended. A magnificent induction-light is obtained by connecting the wires with the tinfoil coating.
- 0·002. The spark between *a* and *b* becomes smaller, and passes with difficulty, but the light between *c* and *d* continues.
- 0·0003. The spark leaps over the distance between *a* and *b* still from time to time, but the light between *c* and *d* continues.

Third Experiment.—I used for this experiment a glass tube 18 millim. wide, bent into the form of a ring of 155 millim. diameter, into which two platinum electrodes were melted nearly at the ends of a diameter. An open glass tube from the outer side of the ring placed it in communication with the mercury air-pump. As in the previous experiments the tube was surrounded by two tinfoil coatings, which were not connected in any way with the electrodes. When the air had been exhausted to a certain degree, the current passed between the electrodes through both halves of the tube, so that the whole tube was illuminated. If, on the other hand, the connecting wires of the apparatus are attached to the tinfoil coatings, no induction-light shows itself. If the exhaustion is pushed to the highest limit attainable with the air-pump, it is impossible for the current to pass between the electrodes, whilst when the conducting wires were removed from the tinfoil coatings the tube becomes luminous immediately. This experiment therefore confirms the former ones. One of the Geissler tubes belonging to the collection of physical apparatus of the Academy of Sciences, a tube 300 millim. long and 20 millim. in diameter, having platinum electrodes melted into the ends, was so far vacuum that the current of the Ruhmkorff's induction-coil would not pass between the electrodes, a distance of 200 millim. On surrounding the tube with tinfoil coating and connecting them with the coil, the tube commenced at once to diffuse a brilliant light, due to the induced currents which traversed it. Consequently we have the same result as before.

Fourth Experiment.—The glass tube used for the first two experiments was employed as follows :—The air was exhausted from it until the current from the induction-coil could no longer leap over the interval between the electrodes. The tube having been closed by means of a hermetically closing

tap, the pump was removed and the spherical conductor of an ordinary electrical machine feebly charged was brought near to it. When the tube was rapidly moved to and fro in the neighbourhood of the conductor, it was illuminated by an intense light, but remained dark if held still or if made to describe a circle round the conductor. This shows clearly that it was an induced current produced in the interior of the tube which caused the light; in fact, no induction-current was produced in the second case, and consequently there was no light. Then the pressure of the air in the tube was increased to about 350 millim. The current of the induction-coil now produced a spark between the electrodes, but no induced light was to be seen on approaching the tube to a charged conductor or on withdrawing it. In this experiment the tube had no coatings of tinfoil, and the portions of the electrodes outside the tube were covered with insulating material.

Corresponding experiments with the ring-shaped tube yielded the same result.

Fifth Experiment.—In a tube like the first described the air was exhausted so far that the current from the induction-coil would not pass between the electrodes. If, then, the one half of the tube was rubbed with a suitable cushion, it began to light up altogether, the electricity contained in the tube being put into motion by means of the electricity produced by the external friction. If, on the other hand, the tube contained air at a high pressure it was impossible to produce light, although the current of the induction-coil easily passed from the one electrode to the other. The same result was obtained with the ring-shaped tube if the air contained in it was sufficiently rarefied. The whole tube became luminous when only one fourth of it was rubbed. These experiments thus completely confirm the previous ones.

We see thus that the admitted fact that an induced current passing through a gas increases in intensity in proportion as the gas is exhausted, but that, if the exhaustion is continued, it decreases, is not explained by supposing that from the moment when a certain limit is reached the resistance increases as the exhaustion is continued. On the contrary, experiments show that the fact in question is to be explained by the increase which takes place in the resistance offered to the electricity in its passage from the electrodes into the gas as the exhaustion of the gas increases, until it becomes so great as to present an insurmountable obstacle to the passage of the electricity. There is thus no experimental foundation for the assumption that an absolute vacuum is an insulator. Since the conducting

power of the gas increases continuously with the exhaustion which it undergoes, even when this is pushed as far as possible, it would be more philosophical to assume that an absolute vacuum is a conductor.

XVI. *On some Electromagnetic Experiments of Faraday and Plücker.* By S. TOLVER PRESTON*.

[Plate II.]

ALTHOUGH it might seem late in the present day to call in question any at all fundamental theoretic conclusions on the subject of magnetism deduced from experiment, yet we know that cases admitting of a *double* interpretation do *sometimes* occur in experimental research ; and precisely on account of their extreme rarity are such cases liable to elude attention and to lead to misapprehensions. About the year 1869, when reading Faraday's 'Experimental Researches,' such an instance attracted my notice, and it recurred some years afterwards while perusing a paper of Plücker's (*Pogg. Ann.* Bd. lxxxvii. 1852, S. 353: "Ueber die Reciprocität der elektromagnetischen und magnetoelektrischen Erscheinungen"). The same ambiguous case occurred both to Faraday and Plücker; and neither appears to have noticed the fact of its being ambiguous (or admitting of a *double* interpretation). Yet the interpretation taken by them, when carried out to its logical conclusions, appears to contradict the theory of Ampère as to the similarity of the helix (solenoid) and magnet in their *chief* properties—a theory which seems to have been hitherto generally accepted.

I will at once proceed to the point in question. Faraday, and also Plücker, adopted quite a peculiar view as to the behaviour of the system of force ("lines of force") about a magnet when *rotated on its axis*; and they set up in this way (led by the experiments) a fundamental distinction of principle between the *translatory* and *rotatory* motion of a magnet in regard to the behaviour of the field of force about it. Faraday himself calls his own view "singular." The following are the words of Faraday relating to this point:—

"When lines of force are spoken of as crossing a conducting circuit, it must be considered as effected by the translation of a magnet. No mere rotation of a bar-magnet on its axis produces any inductive effects on circuits exterior to it. The system of power about the magnet must not be considered as revolving with the magnet any more than the rays of light

* Communicated by the Author.

which emanate from the sun are supposed to revolve with the sun. The magnet may even, in certain cases, be considered as revolving amongst its own forces, and producing a full electric effect sensible at the galvanometer." (Phil. Trans. 1852, page 31.)

Without at once going into the related practical question as to whether a magnet (the rotating globe of the earth, for instance) can, by "revolving amongst its own forces," become charged at the equator and poles with electricity of opposite sign (as Faraday * inferred from this experiment), I would only wish to call the unbiassed attention to the above distinction *in principle* between a *translatory* and a *rotatory* motion, as it seems to me that such a distinction (as a matter of theoretic reasoning) cannot possibly hold. For rotation is surely after all only a particular case of translation, viz. translation *in a circle*. When a magnet rotates on its axis, every part or magnetic point in it is *translated* (in a circle) about that axis. And certainly it will be conceded in regard to Faraday's analogy of the (axially) revolving or rotating sun, the *same* principle (whatever it was) that applied to the behaviour of the "rays of light" of the *rotating* sun, also must apply to the rays of light of the *translated* sun (in its proper motion). For we do not ask whether a given luminous point or portion of the sun's surface is moving in a *curved* or in a *straight* line in order to set fast *the principle* of the behaviour of the rays of light. The same reasoning must therefore apply to the magnet if Faraday's analogy is to hold at all, *i. e.* if (in regard to the inductive effect) the lines of force must be regarded as partaking of the motion of a magnet when it is *translated*, they must partake of its motion when it is *rotated* †. This, therefore, constitutes the first theoretic objection to the view adopted by Faraday, and it seems to me to be in itself conclusive. But, for the sake of further illustration, I will point out how this view clashes with the generally accepted similarity in (at least) *fundamental* properties between the solenoid, or helix, and the magnet.

It seems always to have been taken for granted that when a solenoid, or a simple circular current, rotates on its axis in

* Plücker also inferred it from an analogous experiment, and remarks, "Hier tritt alsdann in den beiden Polen die positive, unter dem Aequator die negative Elektrizität auf, während eine Indifferenzzone zwischen dem Aequator und jedem der beiden Pole liegt." (Pogg. Ann. 1852, p. 357.)

† Or, in other words, if the lines of force emitted by a given portion of a magnetic surface must be considered as partaking of the motion of that portion of surface, when it is *translated* in a straight line, they must equally do so when that portion of surface is translated *in a curved line* (by the rotation of the magnet on its axis).

its own plane, the inductive effect depends only on the *relative* motion between the circular current and neighbouring conducting bodies, and does not depend on whether the circular current or the conducting bodies move. When, for example, a piece of wire *ab* (Pl. II. fig. 1) is placed near a revolving solenoid or simple circular current, *ss*, it is quite indifferent, as regards the inductive effect, whether the circular current revolves about its centre (or axis), or whether, conversely, the wire *ab* is made to revolve about the fixed circular current, in the direction of the dotted line, so long as the *relative* motion between the two is the same. I only suppose that in reference to inductive effects with currents we have to do with *relative*, and not at all with *absolute* motions. If that is true, and also if the magnet is to resemble the solenoid in its *chief* properties (according to the well-known theory of Ampère), then I contend that a magnet in the above case must behave in principle as a solenoid or circular current. Faraday, however (and with him Plücker), holds the opposite. Faraday expresses the view that it is not indifferent whether the magnet or the external conductor moves, that the effect is *not* dependent on relative motion; in short, that “no mere rotation of a bar-magnet on its axis produces any inductive effect on circuits exterior to it. The system of power about the magnet must not be considered as revolving with the magnet” (*loc. cit. suprà*).

I will now consider the experiment whence Faraday derived the above singular (or exceptional) view in regard to a *rotating* magnet. A copper disk *k* (fig. 2) was cemented upon the end of a round bar-magnet *m*, with insulating paper intervening—or, in the words of Faraday:—

“The magnet and disk were rotated together, and collectors (attached to the galvanometer) brought in contact with the circumference and the central part of the copper plate. [In the diagram, the dotted line shows this outer circuit, with the galvanometer *g*.] The galvanometer-needle moved as in former cases, and the direction of motion was the *same* as that which would have resulted if the copper only had revolved and the magnet been fixed. Neither was there any apparent difference in the quantity of deflection. Hence rotating the magnet causes no difference in the results, for a rotating and a stationary magnet produce the same effect upon the moving copper.” (Phil. Trans. 1832, page 183.)

Here Faraday observed (beforehand) a certain deflection of the galvanometer when the copper disk was made *alone* to revolve through the lines of force of the fixed magnet. He observed the *same* deflection when the magnet and copper disk

were revolved *together* (as one whole). Hence he seems to have reasoned that because the deflection remained the same, the conditions must have remained the same; and therefore that the revolving copper disk, even when attached to the magnet (and revolving with it), still continued to cut through the magnetic lines of force of the magnet, precisely as it would have done if the disk *alone* had revolved and the magnet been fixed. Hence Faraday concluded that the lines of force could not possibly partake of the *rotatory* motion of the magnet*; but that the magnet revolved “amongst its own forces.”

But I believe it will be apparent here, on inspection of the diagram of the apparatus, that the same deflection would be equally consistent with the *opposite* supposition (viz. that the lines of force partake of the rotatory motion of the magnet, in the same sense as they partake of the translatory motion of the magnet). For, admitting that the lines of force revolve with the magnet, then *they will intersect the galvanometer-loop circuit* when the magnet revolves on its axis; and this will evidently produce a current of the same direction and magnitude as under Faraday’s singular assumption, which he thought himself forced into by the experiment. This, I think, is clearly one of those *rare* cases of a deceptive *double* meaning to an experimental fact, which on account of their rare occurrence are unexpected, and may consequently sometimes escape the notice of even skilful experimenters. I venture to believe that few will doubt what the true explanation here is after the above elucidation; and only in this way does the contradiction to Ampère’s theory (of the similarity of the properties of the helix and magnet) vanish, as also the great theoretic difficulty of drawing a distinction between *translatory* and *rotatory* motion in regard to the behaviour of the magnetic lines of force.

I conclude, therefore, that the inferred inductive charge produced at the poles and equator of the revolving globe of the earth (as a rotating magnet), by “revolving amongst its own forces,” certainly does not necessarily follow† from this

* After a second perfectly similar experiment to this, with a cylindrical copper cap instead of a disk, Faraday adds “*Thus a singular independence of the magnetism and the bar in which it resides is rendered evident.*” (Page 184.)

† On the other hand, another quite different cause may conceivably conduce to an electric disturbance on the earth’s surface, or, rather, on parts of it which are not rigidly fixed. The *tides*, namely, form a circular band or ring of water through which the earth revolves. These great water elevations are not therefore *fixed* (relatively to the revolving earth) as the globe of the earth itself is; and consequently the magnetic lines of force carried along with the revolving globe can intersect the tides, and

experiment. Whether there are *other* independent grounds for such an assumption, I am not aware, and Faraday does not state them.

It will be seen clearly that Faraday's view involves the strange assumption that although (as an admitted fact), when a copper disk is revolved parallel to and above the pole of a magnet, an inductive effect is produced on the disk [so that its centre and periphery become statically charged with electricity of opposite sign], yet when, conversely, the magnet is made to revolve *with the same relative velocity* below the fixed disk, *no effect* is produced; yet the *relative* motion is really the same in both cases. In fact, Faraday's aspect of the case involves the supposition that the parts of a body at rest (*relatively to the body itself*, when it revolves) can produce an inductive effect on the body.

The following experiment (Phil. Trans. 1832) may possess an interest, as it was considered by Faraday to confirm his view of the static charge produced at the equator and poles of a magnet by its own revolution, and which also led (Plücker especially) to speculations over the cause of the aurora borealis (Pogg. Ann. 1852, p. 357). A cylindrical bar-magnet, *m* (fig. 3), was set by Faraday in axial rotation, and the galvanometer wires were maintained in sliding contact with the equator and one pole of the magnet respectively, as the dotted lines in the diagram indicate. A current was observed; and the cause of this current was referred (in the same way) to a static charge produced by the magnet cutting through its own (internal) lines of force, these lines, or the internal field of force, having been supposed to remain rigidly at rest, while the magnet revolved independently through it. But it will be seen that the current may be simply referred (in an analogous way to the previous example of the copper disk) to the lines of force moving with the revolving magnet, *and intersecting the external circuit formed by the galvanometer wires*. This explanation has the additional advantage of bringing the current formed, whether by the rotation of the magnet on its axis, or by the (inverse) revolution of the wire about the magnet, *under the same cause* (and not under different causes).

so cause a disturbance of the electric equilibrium on the earth's surface of the character imagined by Faraday, and assigned by him to a quite *different* cause. Also the trade winds, which, somewhat similarly, form a kind of air-belt through which the earth revolves, can conceivably conduce in the same way to an electric disturbance, especially as these winds near the equator are laden with moisture, so as to become in some sense conductors.

Twenty years afterwards, in 1852, in order to test the case further, the following experiment was made by Faraday. An insulated wire was bent into the form of a rectangle, $edabf$ (fig. 4), and pushed into the slit or interval between two bar-magnets of oblong cross section, of which fig. 4 (*bis*) shows the end view. The part of the wire buried between the magnets is dotted in the upper figure. The whole was made fast to the axis of a convenient rotary apparatus, so that the whole could be put in motion about the central magnetic axis (common to both magnets). The ends e and f of the wire loop were brought out and attached separately to two insulated metal rings fastened on the axis of rotation, and suitable springs pressed upon these rings, these springs thus serving to carry off any current to a conveniently placed galvanometer (Phil. Trans. 1852, p. 30, &c.). The whole arrangement is accordingly very simple. The magnets with rigidly attached rectangular loop were revolved together (*as one whole*) about the central axis, and no current or deflection was observed at the galvanometer. The absence of current was explained in the following manner. The outer projecting part of the rectangular loop, viz. ade , was supposed by its rotation to cut through the *outer* lines of force of the magnet, which were considered to remain in absolute rest while the magnet revolved. The inner part of the rectangular loop, viz. abf (dotted in the diagram), was supposed, on the other hand, to cut through the *inner* or interior magnetic lines of force, which were also thought to remain in rigid rest, while the (compound) magnet independently revolved through them. In this way it was supposed that two opposite currents tended to be produced in the rectangular loop, and so exactly neutralized each other; and the absence of current in the loop was thus explained. But instead of having recourse to this complicated hypothesis, one can explain the absence of current by the simple fact that the whole system is *at rest* when it revolves as a whole, or there is no *relative* motion between its parts, which are rigidly connected; and without *relative* motion there can be no inductive effect*.

* Faraday considered that this experiment—where magnet and attached wire loop are revolved as *one rigid whole*—proved that the lines of force *within* the magnet were equal in power to those *outside* the magnet; because he supposed that the lines of force remained in *absolute* rest while the system revolved through them, whereby the current generated by the intersection of the *internal* lines of force was considered equal and opposite to that generated by the intersection of the *external* lines of force. Faraday says :—"The results, when the wire and magnet rotated together,

Moreover, it seems very apparent on consideration that when the above double magnet rotates, its lines of force must rotate with it. For imagine *one* of the two magnets (which form the compound bar) to be removed. Then the centre of gravity of the other will now lie outside the axis of rotation. Therefore this magnet and therefore its lines of force will (by the rotation) be *bodily translated* about the axis. For Faraday himself admits that when a magnet (or its centre of gravity) is *translated*, its lines of force accompany it, and would intersect and inductively affect external conductors. The same reasoning now applies to both magnets (if the other be brought back), so that when the whole revolves the lines of force of each bar (and therefore of the whole) are *translated* about the axis, *i. e.* revolve with the magnet (or compound bar), and would influence external conducting bodies. This must, I think, be sufficiently clear; indeed, by holding a sheet of paper with loose iron filings horizontally above the end of the compound bar, one could, no doubt, in a certain sense, *see* the movement of the lines of force by observing the small disturbances produced in the iron filings, as the irregularities in shape or magnetic distribution of the poles pass under the filings when the compound bar revolves. This must be obvious enough, and it will probably be thought superfluous to carry the analysis of this point further. For we are concerned here only with *one* single oversight (depending on a double or ambiguous aspect in the experiments); and as soon as Faraday, in the first instance, thought his view warranted or necessitated by the experimental results, he, of course (consistently), applied it throughout, and so a considerable number of experiments are affected. My object is only to reach the truth, and, whatever it may be, we shall certainly gain by its recognition.

As regards the experiments of Plücker, although the various forms of apparatus specially constructed by him for the determination of this and related questions is generally more complicated than the apparatus of Faraday, yet it is easy to see on going through the drawings that the same oversight, *viz.* the neglect of the influence of the galvanometer wires (or the external galvanometer-loop circuit) on the results, affects all his experiments alike; so that I deem it unnecessary to consider these experiments further here. They are described very completely in the paper in Poggen-

show that these (currents) are perfectly equal to each other" (p. 33). My contention is that this experiment proves nothing, although there may be *independent* grounds for concluding that the lines of force *outside* the magnet are equal in power to those *inside*.

dorff's *Annalen* before referred to. I will only mention one point. In one of Plücker's experiments he used an apparatus consisting of a copper disk, *d* (fig. 5), in which six round bar-magnets were fixed vertically, as the diagram shows. The whole was rotated about the central axis. It was then thought (according to the view adopted by him and previously held by Faraday) that the lines of force did not share the rotation* of each bar-magnet about its axis, although they *did* share the rotation, *i. e.* translation (in a circle), of each bar-magnet about the *axis of the disk* when the latter revolved. Hence it was believed that when the whole system revolved, the lines of force of each magnet remained *axially* in absolute rest, and therefore (virtually) revolved backwards, relatively to the direction of revolution of the disk, thereby intersecting the disk and themselves, and charging up the disk inductively, as it was thought. One sees to what a complicated result this hypothesis leads. The inductive charge of the disk was thought to be proved by touching the axis with one galvanometer wire, and the periphery of the disk with the other, when a deflection was observed. It was overlooked that this deflection is attributable to the passing of the magnets through the galvanometer-loop or circuit as the disk revolves, whereby the lines of force of the magnets successively intersect the wires.

If one wished to illustrate this subject further experimentally, it would be necessary to be on one's guard against a special case, which otherwise might lead to deceptions. This may be best seen by taking a particular example. We will suppose that a magnet, *m* (fig. 6), taken for simplicity the shape of a sphere, is surrounded by a metallic covering, or shell, *h*, with an air-space between. At the poles of the spherical magnet, projections *ns* reach through holes in the shell, by which the magnet can be put in rotation independently—or (when necessary) connected to earth, or to an electrometer. Then, by the rotation of the magnet-sphere about its magnetic axis *ns*, the lines of force (partaking of the motion, *on my view*) will intersect the metallic shell *h*, and therefore strive to generate a current there, such that the parts of the shell opposite the equator and poles of the magnet will be statically charged with the opposite electricities, whose sign will depend on the direction of rotation. The charge on the metallic shell is accordingly just the same as if the magnet had been fixed and

* The lines of force were supposed to behave in this respect as magnetic needles would do, if balanced on pins projecting from the axis of each magnet; then when the whole revolves, the needles (pointing steadily north and south) would *virtually* revolve backwards, or remain *axially* at rest in space.

the shell had been rotated in the opposite direction about the magnet (or, on my view, the charge depends on the *relative** motion between the magnet and shell, and not on which revolves). When the shell revolves about the magnet, the existence of the charge upon it is admitted, and is dependent on well-known principles; but when the magnet revolves, the existence of the charge upon the shell would have been denied by Faraday—*although the relative motion (between magnet and shell) is the same in both cases*. For Faraday considered that “no mere rotation of a magnet on its axis produces any inductive effect on circuits exterior to it.” But Faraday thought that the rotation of a magnet on its axis could produce an inductive effect *on itself*, or that the magnet could become statically charged by “revolving amongst its own forces.” Although I contend that Faraday’s experiments do not prove this static charge, I think I can show how (from another cause) such a static charge could come on the magnet in the *present special case* (fig. 6) without revolution “amongst its own forces.” For it is apparent that if a static charge is produced on the shell by the axial revolution of the magnet (whose lines of force partake of its motion and intersect the shell), then this charge will act by *static electric induction*—like a Leyden jar—across the layer of air, and call forth an opposite static charge (or distribution of electricity) on the corresponding parts of the magnet facing the shell. Unless this fact (deducible beforehand from the considerations here set forth) be kept in view, this peculiar static charge may mislead, and make one think that the magnet could here have become charged in no other possible way but by “*revolving amongst its own forces*.”

Since the first draft of this paper was written, I have observed that this question is touched upon in the very complete work of Prof. Wiedemann (*Die Lehre vom Galvanismus*), where there are also some references, viz. to Beer, Pogg. Ann. Bd. xciv. S. 177 (1855); Nobili, Pogg. Ann. Bd. xxvii. S. 421 (1833), &c. But on referring to these papers, I find that they practically leave the question open (or have done nothing to decide it), and I find no mention of the possible *double* (or ambiguous) meaning of Faraday’s fundamental experiment, upon which it may be said the whole question hinges. And

* It is curious here to remark that, on Faraday’s view (that the “system of power about the magnet must not be considered as revolving with the magnet”), it would follow that the inductive effect on the shell produced by revolving it concentrically through the lines of force of the magnet would not be altered *if the magnet itself were put in motion at the same rate as the shell* (so that the two were in *relative rest*); or it would be necessary to conclude from this view of Faraday’s that the inductive effect between two bodies is the *same* by *relative rest* as by *relative motion*.

it would seem desirable that any diverse views on a tolerably fundamental question of electromagnetism should cease, or a satisfactory general agreement be arrived at; which may be facilitated by an unbiassed discussion of the subject in the interests of truth.

Subsequent Addendum.—Besides the *practical* aspect of this question as relating to the revolving earth (as a magnet), and to the identity in fundamental qualities between the magnet and the helix (involving the validity of Ampère's theory), there is another practical application of the matter.

It seems, namely, extremely difficult (if not impossible) on Faraday's view of "*a singular independence of the magnetism and the bar in which it resides,*" to explain how a bar-magnet is caused to rotate on its axis when one terminal of a galvanic element is connected with one pole of the magnet, and the other terminal with the equatorial part of the magnet (the magnet being suspended freely, and the terminals of the galvanic cell maintained in sliding contact with its equator and pole). Thus, suppose the galvanometer, *g*, of fig. 3 to be removed, and a battery or galvanic element substituted for it. Then the magnet is observed to revolve (as is well known). But how is the magnet to be made to revolve by the action of the current on its lines of force if the lines of force are *independent* of (the rotary motion) of the magnet? The lines of force may be made to revolve no doubt (by the current); but how are they to drag the magnet round with them when they are *independent* of it?—indeed so independent (as Faraday supposes) that the magnet can be *actually made to rotate through its own lines (or field of force)*. If the rotation of the magnet cannot cause the field of force to rotate, how can (conversely) the rotation of the field of force cause the magnet to rotate; in other words, how is the rotation of the magnet by the action of a current to be accounted for on Faraday's hypothesis? It appears that, on the opposite hypothesis, the explanation is simple; viz. the rotation of the magnet is then easily referable to the reaction of the current in the external portion of the circuit upon the *external* lines of force of the magnet (whereby these external lines of force are caused to revolve), and, being *dependent* on the magnet, the magnet is caused to revolve with them.

Heatherfield, Bournemouth, Dec. 1883, and
Paris, February 1885.

XVII. *Notices respecting New Books.*

Absolute Measurements in Electricity and Magnetism. By ANDREW GRAY, M.A., F.R.S.E., Chief Assistant to the Professor of Natural Philosophy in the University of Glasgow [now Professor of Physics in University College, Bangor]. London: Macmillan and Co., 1884 (pp. xiv + 194; sm. 8vo).

THE chief function of this little book is to describe the principles and methods of electrical measurement, as introduced and modified by Sir William Thomson, and in use at Glasgow University. With some slight additions it is a reprint of articles published in 'Nature'; and it contains in a convenient form some very useful practical details and hints, which are evidently given by one thoroughly conversant with the apparatus described.

The first chapter contains an account of the usual method of determining the earth's horizontal intensity and the moments of magnets, carried out with extemporized but quite satisfactory appliances—appliances, indeed, in many respects more in accord with the demands of theory than are the present Kew instruments. Then, after a chapter of definitions, comes the tangent galvanometer, with its elementary theory fully worked out. Then a full and instructive description of Sir W. Thomson's two new "graded" galvanometers for measuring potential and current respectively. The range and versatility of these instruments render them very serviceable in a laboratory, though they are probably too complex and delicate for workshop manipulation; and the account of their uses, by one who is so thoroughly familiar with their working as is Mr. Gray, gives the little book a unique value.

Methods of measuring resistance, some of them well known, others less known, are given pretty fully; and a very interesting chapter on the measurement of intense magnetic fields concludes the practical portion of the book.

Not many electricians know how to measure the intensity of the field between the poles of an electromagnet, but it is a matter of vital importance in the construction of dynamos; and if the methods here described enable measurements of magnetic fields to be carried out with anything like the same glibness as is now the case with currents and resistances, the book will have done good service and much aided the practical development of the science. This is indeed too much to expect, but a beginning can now be made; and the still-needed practical instrument, convenient and simple in use, yet able to effect measurement of fields with reasonable accuracy, will before long be invented.

Concerning the more theoretical portions of the book, they are not different from what is already common property; nevertheless, isolated as they here are from more difficult matter, and embodied in a small compass, these portions will be found very useful for college classes such as ordinarily go by the name of "Senior."

The little book, in fact, occupies a distinct niche of its own, between elementary popular volumes on the one hand and advanced or bulky treatises on the other. Not aiming at too much, it accomplishes what it does aim at in a sound and practical way.

O. J. L.

Exercises on Electrical and Magnetic Measurement. By R. E. DAY, M.A. New Edition. Longmans, Green, and Co. (Pp. 188.)

THE object of this work is explained by the quotation in the preface from a lecture by Sir W. Thomson:—"In physical science a first essential step in the direction of learning any subject is to find principles of numerical reckoning and methods for practically measuring some quality connected with it. I often say that when you can measure what you are speaking about and express it in numbers, you know something about it; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of *science* whatever the matter may be." We have here a collection of 580 problems in Electricity and Magnetism; and any student who has worked through these examples, or who is able to correctly work any of them, may be satisfied that he is possessed of a thorough knowledge of the elementary portions of these sciences. The book is, however, something more than a mere collection of problems. Many of the problems, some at least in each section, are worked out in full, so that the book forms almost a treatise upon the subject. For example, in treating of Ohm's law, we have a proof (possibly not altogether as simple as it might be), involving only simple algebra, of the condition for maximum current from a given number of cells, viz. that they must be so arranged as to have an internal resistance as nearly as possible equal to the external resistance. Most elementary textbooks simply state that this can be proved by the use of the differential calculus; but it is much more satisfactory to the student to have a proof given him.

The book is prefaced by an explanation of the C.G.S. system of units; and whilst most of the problems are given in the practical units—the volt, ampere, and ohm—yet a sufficient number are expressed in the theoretical units, to make sure that the student is able to employ either system, and understands the relationship between the two systems. We can thoroughly recommend this work as one most useful to the student.

Numerical Tables and Constants. By SYDNEY LUPTON, M.A. Macmillan and Co. (Pp. 96.)

THIS is a *small* book containing a very *large* amount of useful information, and science teachers will be grateful to Mr. Lupton for this very useful compilation. The book will, however, very probably be found to be in use both too large and too small. The

analyst, for example, will be disposed to think the tables which he most frequently requires too limited; while he may be disposed to think that tables of the values of Hebrew, Greek, and Roman measures, of the number of vibrations per second which constitute F# or Db, of Geological formations, or of Tide-tables, are (to him) useless matter. In view of the small size of the present (first) edition, we trust that in a second edition many of the tables may be extended. For example, it would be most useful to have, instead of the present page of 4-figure logarithms, a table of 5-figure logarithms; the table of trigonometrical ratios might with great advantage be enlarged; and the same remarks apply to the tables of boiling-points, the table for the conversion of temperatures, and the table of lengths of rivers. We would further suggest that in the table of rotation of polarized light the effect of temperature should be taken into account; and that the comparison of electromotive forces for various batteries should be supplemented by some statement of their usual internal resistances. A little more explanation might occasionally be given; *e. g.* in the first table, at present, stands the statement that when $n = 2$, $\frac{1}{n} = 50,000$, which of course is not intended. Still these are small matters which can easily be set right in a second edition, which will no doubt soon be required.

XVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 64.]

December 14, 1884.—W. Carruthers, Esq., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. "On the South-western Extension of the Clifton Fault." By Prof. C. Lloyd Morgan, F.G.S., Assoc. R.S.M.

This fault cuts across the strata out of which the Avon gorge has been excavated, a little north of the Suspension Bridge. According to the author's estimate the throw of the fault is, on the Gloucestershire bank, somewhat less than 1200 feet, and somewhat more than 1100 feet on the Somersetshire bank. The difference of nearly 100 feet the author considered to be, in part at least, due to the dying-out of the fault to the west. Taking as a datum-point the intersection of the line of fault and the line of high-water mark, the rocks relatively shifted downwards are, on the Gloucestershire bank, Mountain Limestone 730 feet, Upper Limestone Shales 470 feet. According to this estimate there would be 130 feet of Upper Limestone Shales

above high-water mark, above which beds of Millstone Grit would be brought down. This accords with observed facts. On the Somersetshire bank the beds brought down below high-water mark are Mountain Limestone 770 feet, Upper Limestone Shales 330 feet. According to this estimate there would be 270 feet of Upper Limestone Shales above high-water mark, which would thus leave little or no room for Millstone Grit to be brought down to the surface. Nor has the author succeeded in finding any sign of this rock on the Somersetshire side.

Owing to the fact that *softer* Upper Limestone Shales are brought down by the fault, its westward extension may be traced by a *line of depression* resulting from the greater erosion of these softer beds. In the map which the author exhibited, a triangular wedge of Upper Limestone Shales, brought down by the fault, had its apex near Hill Farm (see Survey Map), and its base abutting on the Triassic beds south of Durdham Down. If the fault do not tend to die out westwards, the apex of this triangle must be placed further S.W., for which there is no evidence, while there is some against it. The southern side of the triangle marks the line of fault. Further west the author believed that evidence exists of the faulting-down of a wedge of Mountain Limestone into the Lower Limestone Shales.

2. "On the Recent Discovery of Pteraspidian Fish in the Upper Silurian Rocks of North America." By Prof. E. W. Claypole, B.A., B.Sc., Lond., F.G.S.

3. "On some West-Indian Phosphate Deposits." By George Hughes, Esq., F.C.S. (Communicated by W. T. Blanford, Esq., LL.D., F.R.S., Sec. G.S.)

Some West-Indian specimens of phosphates were exhibited, in reference to which the author called attention to a description by Dana of an instance in which the carbonate of lime in fragments of coral was partially converted into phosphate, and also to the apparent alteration of limestone rock into phosphate of lime in Barbuda Island by the action of water draining a guano-like deposit of bats' dung in a cave. A specimen of the phosphate of lime thus produced was exhibited.

In Aruba Island the process of conversion of coral-rock into phosphate of lime has been in operation on so extensive a scale that the deposit is being largely worked for shipment. The alteration is probably due to the action of water containing soluble phosphates derived from the excrements of sea-birds (guano). Of this guano no trace remains; but the fragments of coral in the underlying rock have been altered into a substance containing from 78 to 80 per cent. of phosphate of lime; and the deposit, as shipped, contains 35·7 per cent. of phosphoric acid, equal to 77·9 per cent. of tribasic phosphate.

Reference was also made to some other West-Indian phosphate-

deposits formed of bones, and to iron and alumina phosphates found in Redonda, Alta Vela, and Testigos Islands.

4. "Notes on Species of *Phyllopora* and *Thamniscus* from the Lower Silurian Rocks, near Welshpool, Wales." By George Robert Vine, Esq. (Communicated by Prof. P. Martin Duncan, F.R.S., F.G.S.)

January 14, 1885.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S.,
President, in the Chair.

The following communications were read :—

1. "The Metamorphism of Dolerite into Hornblende-schist." By J. J. Harris Teall, Esq., M.A., F.G.S.

The author referred to two dykes which occur in the neighbourhood of Scourie, Sutherlandshire. Their general direction is N.W. and S.E.; and as the prevalent strike of the gneissic banding is, roughly speaking, N.E. and S.W. (not N.W. and S.E. as is usually the case with the Hebridean gneiss of Sutherland), there can be no doubt as to their true character. It is impossible to regard them as bands in the gneiss.

The author's observations referred more particularly to the southern dyke, which is well exposed on the sea-shore on the north side of Scourie Bay, and especially at the promontory named Creag a' M'hàil. The rock of this dyke occurs in two very different forms, which are proved by chemical analysis to agree very closely in composition. The one is a typical dolerite (German "diabase"), the other a typical hornblende-schist. Transitions from one form to the other are easily traced. Veins of quartz and, in one case, a vein of nearly pure andesine were observed in the dyke. An analysis of the latter mineral was given in the paper.

The original rock is composed of pyroxene, lath-shaped feldspars, titaniferous iron-ore, frequently in skeleton rhombohedra, and apatite. This may be traced by gradual stages into a massive rock, in which the augite has been completely replaced by green hornblende, and the original microstructure entirely destroyed. The modifications in the feldspar-substance have not been worked out; but the resulting product is frequently a colourless mineral, without any definite external boundaries, and often difficult to distinguish from quartz. In many cases the author found himself unable to decide whether the colourless grains occurring in the altered rock were quartz or feldspar, or some mineral resulting from the modification of feldspar.

The massive rock in places passes into a typical hornblende-schist, and the transition may be often seen in a hand specimen. The schist is composed principally of crystalline grains of green hornblende, a colourless mineral or minerals (quartz and feldspar?), titaniferous iron-ore, sphene, and apatite. The hornblende grains show

no definite crystalline form, and they have their longest axes, which correspond for the most part with the vertical axes of hornblende crystals, lying parallel to each other in the plane of schistosity. In fact the different grains show a striking amount of uniformity in their arrangement in the mass of the rock, the corresponding axes of elasticity lying, as a rule, approximately parallel to each other. The titaniferous magnetic ore is never present as skeleton rhombohedra, but is drawn out into long strips in the plane of schistosity, and frequently more or less surrounded by granules of sphene. Apatite occurs in broken prisms. Turbid grains of felspar are frequently, but not always, present.

The author argued that the schist has been produced by metamorphic action subsequent to the consolidation of the original rock. This metamorphic action has sometimes resulted only in the molecular rearrangement of the different constituents, but where the unequal distribution of stress has given rise to a kind of plastic movement in the mass a typical crystalline schist has been produced.

The distribution of foliation in the dyke is not in accordance with any simple law. Sometimes it is parallel to the sides and sometimes it runs across the dyke. In the latter case it agrees, as far as strike is concerned, with the prevalent direction of the gneissic bands in the district. In many places and for considerable distances no trace of foliation can be detected.

In conclusion the author pointed out that the metamorphism of basic igneous rocks into hornblende-schist had been discussed by many previous writers. He also acknowledged his great indebtedness to Prof. Lapworth.

2. "Sketch of the Geology of New Zealand." By Captain F. W. Hutton, F.G.S., Professor of Biology in the Canterbury College, University of New Zealand.

The paper commenced with some general remarks on the importance and variety of the geology of New Zealand, and on the progress made in the investigation of the islands. The author then proceeded to the question of the classification of the sedimentary strata, which he arranges in the following local systems:—

Systems.	Probable age.
Recent.	Recent.
Pleistocene.	Pleistocene.
Wanganúi.	Newer and Older Pliocene.
Pareóra.	Miocene.
Damarú.	Oligocene.
Waipara.	Upper Cretaceous.
Hokanúi.	Lower Jurassic and Triassic.
Maítaí.	Carboniferous.
Tákaka.	Silurian and Ordovician.
Manapóuri.	Archæan.

Most of these systems are divided into several local series.

The general geological structure was then treated. The south island of New Zealand was shown to be traversed from near the southern extremity to Tasman's Bay by a curved anticlinal, convex to the westward; and the strata to the east of this axis are thrown into secondary folds, which mainly affect the beds older than Tertiary. A great north-and-south fault occurs west of the anticlinal.

The north island is very different. It is traversed by a narrow ridge, the country northward of which is broken by three great volcanic cones, Mount Egmont, Ruapehu, and Tongariro near the centre of the island. The oldest rocks seen south of Cook's Straits are not repeated to the north, and a fault may traverse the Straits.

The rock-systems up to the Hokanui inclusive are similar in lithological character throughout New Zealand, and appear to have been formed on the shore of a continent with large rivers. The higher systems, with the exception of a few coral-reef limestones, are locally variable and may be considered insular.

The relative distribution of sedimentary and eruptive rocks was briefly noticed, and the occurrence of some useful minerals mentioned. No workable coal is found below the base of the Waipara system.

A description of the different systems, and of the series into which they are divided, followed, commencing with the oldest. The distribution, lithology, and thickness of each system were noticed briefly, and lists of the most important fossils were added. The eruptive rocks associated with each system were next noticed in the same order; and the paper concluded with notes on the distribution of volcanic rocks in the north island, on hot springs, and on the minerals found in New Zealand.

3. "The Drift-deposits of Colwyn Bay." By T. Mellard Reade, Esq., F.G.S.

The author, after referring to a former paper (Quart. Journ. Geol. Soc. vol. xxxix. p. 111), described the drifts of Colwyn Bay as forming cliffs towards the sea, and thinning out inland, forming a crescent-shaped deposit, which lies for the most part upon Silurian rock of Wenlock age, while the two headlands of Colwyn Head and Rhos Point are denuded remnants of Carboniferous Limestone. He distinguished three divisions in the drifts:—1. Bluish-grey Till, composed of disintegrated Silurian and Carboniferous rocks, and full of striated fragments of slaty rock, with few granite boulders; its surface is unevenly denuded. 2. Brown Boulder-clay representing the Low-level Boulder-clay and Sands, overlying the former, evidently a marine deposit, and containing shell-fragments. To this bed the author referred most of the erratics found on the shore, which included boulders of Eskdale and Scotch granites; and 3. Rearranged gravels. The line of demarcation between numbers 1 and 2 is particularly clear.

The author considered the Bluish-grey Till to be here, as else-

where, chiefly, if not entirely, made up from the denudation of the local rocks, the material being brought down by the action of ice and snow from the high ground lying inland; while the overlying deposit of brown Boulder-clay is part of the great sheet of Low-level Boulder-clay and Sands which occupies the plains of the north of England from the mountains to the shores of the Irish Sea, and, like the more sandy Boulder-clays near Liverpool, its materials were probably derived chiefly from Triassic rocks, which occur in the neighbouring Vale of Clwyd, mixed with material derived from other rocks, and especially with argillaceous matter from the underlying Till. The granite boulders contained in it were carried to where they lie by floating ice.

XIX. *Intelligence and Miscellaneous Articles.*

A NEW METHOD OF DETERMINING THE CONSTANT OF GRAVITATION. BY ARTHUR KÖNIG AND FRANZ RICHARZ.

IN the older experiments for determining the constant of gravitation the pendulum and the torsion-balance were used as measuring-instruments. Both apparatus were, however, far excelled by the balance which was first applied to problems of gravitation by Herr von Jolly*. He counterbalanced one and the same mass in one case by placing weights in a scale-pan on the same level, and in another case by placing weights on a scale-pan at a depth of 21 metres, and connected with the upper one by a wire. The difference represented the decrease of gravity with height. He then arranged below the lower pan a lead ball weighing 5775 grammes, and again determined the corresponding difference. The increased difference represented the attraction of the lead ball on the weights in the lower scale; for the lead ball, as experiment showed, exerted no measurable action on the upper scale-pan. The most material sources of error are the unavoidable differences in temperature arising from the height of the place of observation, as well as the friction due to currents of air on the wire of 21 metres.

Quite independently of each other we have arrived at a method in which a fourfold attraction of the mass of lead used in measuring by the balance comes into play, and, moreover, differences in temperature and currents of air can be almost entirely avoided.

In the middle of the horizontal surface of a parallelepipedal block of lead a balance is so arranged that its pans are just over the surface. Below each pan the block is bored through vertically, and by means of two rods passing through these holes two other pans are so suspended from the upper pans that they are just below the block.

* *Abh. der Kön. bayer. Akad. der Wiss.* II. Class, vols. xiii. and xiv.; and Wiedemann's *Annalen*, vol. xiv.

A mass m on the upper right pan is counterpoised by a weight m_u placed in the lower left pan. The same mass m is then placed in the lower right pan, and is counterpoised by weights m_o placed in the upper left pan. For simplifying the subsequent scheme of calculation, let us assume that the absolute value of the vertical components of the accelerating forces which the block exerts on the upper and lower scale is the same; let it be denoted by k . If g_o and g_u represent the values of gravity at the upper and lower scales respectively, we have for the two weighings the equations

$$m(g_o + k) = m_u(g_u - k),$$

$$m(g_u - k) = m_o(g_o + k);$$

from which we get

$$\delta_m = m_u - m_o = \frac{m(g_o + k)^2 - (g_u - k)^2}{(g_o + k)(g_u - k)}.$$

If now we put $g_u = g_o + \gamma$, then, seeing that k and γ are very small compared with g_o ,

$$\delta_m = \frac{2m}{g_o} (2k - \gamma).$$

The magnitude γ is to be determined by weighings, which are to be made in the same balance in the same way before building up the lead block. If m stands for the same mass as above, and m'_u and m'_o correspond to the above values m_u and m_o , we have for two such weighings

$$mg_o = m'_u g_u,$$

$$mg_u = m'_o g_o.$$

From this follows

$$\delta'_m = m'_o - m'_u = m \frac{g_u^2 - g_o^2}{g_u g_o}.$$

If, now, we put again

$$g_u = g_o + \gamma,$$

then

$$\delta'_m = \frac{2\gamma}{g_o} m,$$

and therefore

$$\gamma = \frac{\delta'_m g_o}{2m}.$$

If we introduce this value into the expression for δ_m , we get

$$\begin{aligned} \delta_m &= \frac{2m}{g_o} \left(2k - \frac{\delta'_m g_o}{2m} \right) \\ &= \frac{4mk}{g_o} - \delta'_m. \end{aligned}$$

Hence

$$k = \frac{g_o}{4m}(\delta_m + \delta'_m).$$

We therefore obtain k from the measurable magnitudes g_o , m , δ_m , and δ'_m ; in which, as estimating the accuracy, it may be pointed out that δ_m and δ'_m are both positive.

If, now, V is the potential of the block of lead, G the constant of gravitation, z the vertical coordinate, then

$$k = G \frac{\partial V}{\partial z}.$$

The differential quotient $\frac{\partial V}{\partial z}$ may be calculated from the known dimensions of the parallelopipedon, and the position of the pans. Our observations give the constant of gravitation G , and therefore also the mean density of the Earth.

We have already taken the preliminary steps for the experimental execution of this method. We think of using a mass of lead which has about twice the attraction of the ball used by Herr von Jolly; the determination of k may therefore, other things being equal, be made with eight times the certainty. Moreover, from the considerably smaller distance of the upper from the lower scale-pans, $1\frac{1}{2}$ to 2 metres, we are in a position to exchange the weights within a closed case by our automatic arrangement, by which currents of air and differences of temperature are almost entirely avoided. We may therefore accept with certainty a far greater accuracy for our determinations.—*Sitzungsberichte der Akad. der Wissenschaften zu Berlin*, December 1884.

RESULTS FOR USE IN CALCULATIONS WITH MANOMETERS WITH COMPRESSED AIR. BY E. H. AMAGAT.

I gave some years ago the numerical results necessary for calculations with compressed air between 20 to 30, and 430 atmospheres; but these results were not reduced to the standard pressure as starting-point, partly because my series began at pressures higher than those at which Regnault's series end, and partly because these latter series were obtained at a temperature which is too different from that at which I worked. In all the researches in which I have hitherto determined the pressure with a nitrogen-manometer, I took this difference into account, by a method which it is needless to describe here. On the other hand, the results obtained by M. Cailletet and by myself exhibit such differences as to make a verification necessary.

I have made this verification for air and nitrogen up to 85 atmospheres; and at the same time I have made the determinations necessary for referring the series to the normal pressure which is indispensable in the construction of manometers. These experiments were made in one of the towers of the church of Fourvières, where, thanks to the kindness of M. Sainte-Marie Perrin, the architect of the monument, I had at my disposal a vertical height of 63 metres during the whole time necessary for frequently repeating the series. I retained the method I had used at the shaft at Verpilleux, but with some improvements. The glass manometers were so arranged that the gas occupied at least the value of 500 divisions of the stem; the diameter of the iron tubes was double; the conditions were favourable for exactly reducing the volumes of mercury to zero.

The various series agreed perfectly. For nitrogen the results at which I arrived are almost identical with those of my former experiments. The differences do not in general exceed a thousandth of the total pressure, except in one point of the curve which I had already noted as irregular. The differences with M. Cailletet's numbers exist still; between 40 and 60 metres they correspond to an error of more than 2 metres of mercury.

In the following table the values of the product pv for nitrogen and air are for the temperature 16° ; they are compared with the standard pressure, for which they are supposed equal to unity.

Pressure in metres.	Nitrogen, pv .	Air, pv .	Pressure in metres.	Nitrogen, pv .	Air, pv .
0.76	1.0000	1.0000	45.00	0.9895	0.9815
20.00	0.9930	0.9901	50.00	0.9897	0.9808
25.00	0.9919	0.9876	55.00	0.9902	0.9804
30.00	0.9908	0.9855	60.00	0.9908	0.9803
35.00	0.9899	0.9832	65.00	0.9913	0.9807
40.00	0.9896	0.9824			

The deviations for 20 metres are considerably lower than those which had been given by Regnault, which is explained by the fact that Regnault had worked at 4° . For nitrogen the minimum of the product pv corresponds to a pressure of 42 metres; in my first researches it was 45 metres. For air it is 59 metres. It must moreover be remarked that the variation of pv is here so small near the minimum, that an unimportant difference in the pressure may displace the minimum ordinate through several metres.

Within the limits of the above table we may either use air or nitrogen for filling the manometers; for higher pressures it will be preferable to choose nitrogen, which has been directly studied. It will be sufficient to reduce proportionally all the numbers which

I gave up to 430 atmospheres to have them at the normal pressure.

For pressures above 430 atmospheres we may obtain exact results by using hydrogen. I have in fact shown that for this gas the curve representing the values of pv is a straight line; we may without hesitation prolong this line to far higher pressures; this is what I am doing in researches in which I have worked under pressures of several thousand atmospheres, and of which I shall before long give an account to the Academy.—*Comptes Rendus*, December 8, 1884.

ON GAS-ENGINE INDICATOR-DIAGRAMS.

To the Editors of the Philosophical Magazine and Journal.

Siemens Brothers and Co., Limited,
Telegraph Works.

Woolwich, Kent.,
January 16, 1885.

GENTLEMEN,

I enclose a letter from Professors Ayrton and Perry correcting a previous paper of theirs, for which I presume you will find room in your next publication.

Yours truly,
F. JACOB.

Technical College, Finsbury, E.C.,
October 9, 1884.

DEAR SIR,

Mr. F. Jacob, in a letter addressed to Dr. Guthrie and forwarded to us, has kindly drawn our attention to the fact that the method of observation of loss of heat described by us in the Postscript to our paper "On Gas-Engine Indicator-Diagrams," and which was used in obtaining the results referred to in the postscript, is not, as we thought, new, having also been used by the late Sir W. Siemens, and described by him in his paper "On the Dependence of Radiation on Temperature," read before the Royal Society on April 26, 1883; to whom of course we hasten to cede the priority.

We may add that Mr. J. T. Bottomley, in his paper just published in the Proceedings of the Royal Society, "On the Permanent Temperature of Conductors through which an Electric Current is passing" &c., has also described the use of the same method, and he tells us that he, like ourselves, was unaware of Sir W. Siemens's paper.

We are, &c.,

W. E. AYRTON,
JOHN PERRY.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MARCH 1885.

XX. *On the Seat of the Electromotive Forces in the Voltaic Cell.*
By PROFESSOR OLIVER J. LODGE, D.Sc.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE accompanying tract is an expansion of a paper drawn up as the opening of a discussion on Contact Electricity by Section A of the British Association at the Montreal Meeting. It is in five portions or chapters. The first of these [printed this month] is mainly historical; in the second and third portions theoretical views are considered, and my own are given at some length in accordance with the kind advice of Sir W. Thomson; the two remaining chapters are occupied with cognate thermoelectric phenomena, with the theory of the simple voltaic cell, which singularly enough is pretty complicated and very little understood, and with a discussion on the size of atoms. Although the paper is being printed in the annual volume of the Association, it has been suggested that a further publication of it might be useful in order that Physicists may have a better and more leisurely opportunity of contributing to the discussion of a matter which must have occupied the attention of every one more or less, which branches out into several interesting and important developments, and which, as a matter of controversy, has remained singularly long unsettled. I will therefore ask you to find room for it in your Journal, in the hope that the discussion may be there conveniently continued and properly closed.

I am, Gentlemen,

Your obedient servant,

University College, Liverpool,
December 1884.

OLIVER J. LODGE.

Phil. Mag. S. 5. Vol. 19. No. 118. March 1885. M

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1. The subject chosen for the present discussion illustrates in a remarkable way the need for such conversations. It is scarcely credible, at the present rate of progress, that eighty-four years after the discovery of the voltaic pile, opinion should still be utterly divided as to the seat of the main E.M.F. in it. I venture to hope that it may now be decided, and a substantial agreement arrived at with respect to it. My business is to open the discussion; but it so happens that for some seven or eight years I have believed myself to see more or less clearly to the root of this particular matter, and a laborious review of the literature of the subject has only strengthened my conviction. Having, therefore, strong and definite views I can hardly help letting them appear; and without assuming prematurely that these views are agreed with, they may yet serve as a link with which to connect the facts and the multifarious observations thereon.

In the course of my reading on the subject I have found only two great and epoch-making papers, that of Volta in 1801, and that of Sir William Thomson in 1851. The other contributions are some of them keen, like those of Faraday and Clerk Maxwell; some of them laborious, like those of Hankel and Ayrton and Perry; but none contain anything essentially and powerfully new except those two: unless, indeed, we include in the subject the immensely important phenomena of Seebeck and of Peltier, and Faraday's fundamental law of electro-chemical decomposition.

Volta* showed that when two metals were put into contact and separated, the insulated one was charged with electricity sufficient to make gold leaves diverge. He also stated that the contact force between any two metals was independent of intermediate metals, so that the metals could be arranged in a definite numerical series; and he gave the first series of the kind:—



* Volta, *Gehler's Wörterbuch*, iv. p. 616. See also a carefully edited version, *Annales de Chim.* 1 ser. xl. p. 225 (1801).

Moreover, he started a hypothesis to account for the action, a sort of impulsion or attraction of electricity by matter—an idea subsequently elaborated by Helmholtz. Fabroni* objected to Volta's explanation of his experiment. He denied contact force, and considered that the electricity was developed by chemical action.

Then the fight began, and lasted on and off some half-century. On the one side were Volta, Davy, Pfaff, Péclet, Marianini, Buff, Fechner, Zamboni, Matteucci, and Kohlrausch. On the other were Fabroni, Wollaston, Parrot, Ørsted, Ritchie, Pouillet, Schönbein, Becquerel, De la Rive, and Faraday.

It was not all fighting: part of it resulted in a more thorough investigation of voltaic phenomena; and very often the original point of dispute was lost sight of, and Volta's fact itself was doubted in the eagerness to disprove Volta's explanation. The experiments of Pfaff and Péclet†, however, fairly well established the correctness of his observation; and Kohlrausch showed how, by means of a Daniell's cell combined with a condenser, to measure Volta forces absolutely, thus inventing a method which has been employed with modifications by Hankel, by Gerland, by Clifton, by Ayrton and Perry, by Von Zahn, and by most other experimenters on the subject‡.

* Fabroni, *Journal de Physique de l'Abbé Rozier*, xlix. p. 348.

† Péclet on the Contact of Good Conductors: *Comptes Rendus*, 1838, p. 930; Pogg. *Ann.* xli. 1839, p. 346; *Ann. de Chim.* 1842 and 1841, 3 ser. ii. p. 233.

Pfaff, Letter to Gay Lussac: *Ann. de Chim.* 2 ser. xli. p. 236 (1829).—Pfaff: For and against the production of Electricity by Chemical Processes, as a consequence of some experiments on the E.M.F. of liquids and metals: Pogg. *Ann.* xli. 1840, pp. 110 and 197.—Pfaff: Experimentum crucis in favour of the Contact Theory: Pogg. *Ann.* liii. 1841, p. 303. This crux is on p. 306, and consists in substituting ZnSO_4 for H_2SO_4 in a Grove cell, and showing that the current through a thin wire galvanometer is stronger than before. This, he says, leaves no further shift or evasion (Ausflucht) for the chemical theory. It is a fact we have grown accustomed to, but it is rather surprising, that the E.M.F. given by ZnSO_4 should be even higher than that given by H_2SO_4 . A convenient "Ausflucht" could nevertheless be provided for the chemical theory by pointing out that the combustion heat $\text{Zn}, 2\text{NO}_3$ is greater than $\text{Zn}, \text{SO}_4 - \text{H}_2\text{SO}_4 + 2(\text{H}, \text{NO}_3)$, if indeed the fact be so. Another shift is to talk about basic sulphate and the sourness of ZnSO_4 ; another is to use the word "dissociation."

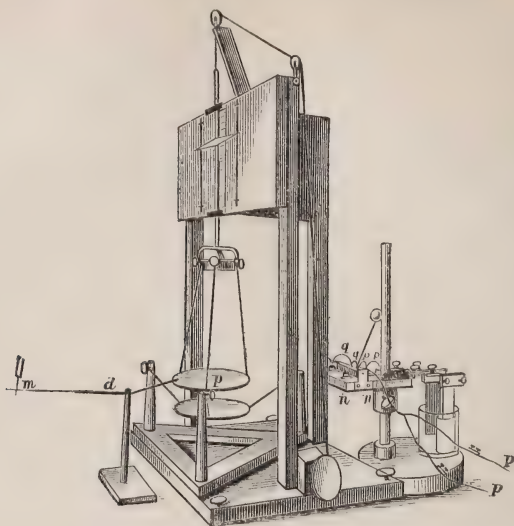
‡ Kohlrausch's method consisted in bringing the plates of the two metals close together, connecting them by a wire for an instant, separating them, and putting one into connection with a Dellman electrometer, the other with the earth. The observation is repeated with a Daniell in the connecting wire, first one way, then the other. Thus three equations are obtained, $M/M' = ka$, $D + M/M' = k\beta$, $D - M/M' = k\gamma$;

whence

$$M/M' = \frac{B - \gamma D}{B + \gamma}, \quad \text{or} \quad \frac{a}{\beta - a} D.$$

(Pogg. *Ann.* vols. lxxv. p. 88, lxxxii. pp. 1 and 40, and lxxxviii. p. 465, 1851 and 1853.) He gets his results much lower than later experimenters; only $\frac{1}{2}$ a volt for Zn/Cu , and $\cdot 58$ for Zn/Pt .

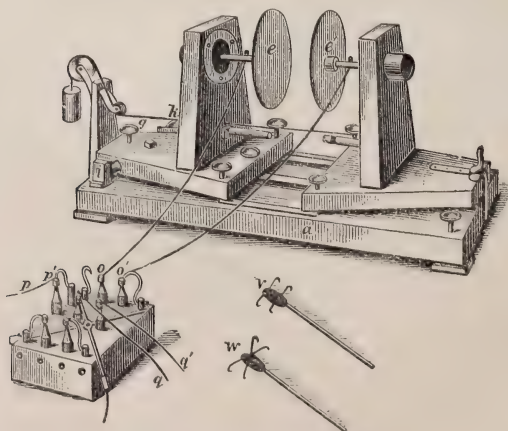
Fig. 1.—Kohlrausch's Early Form of Condenser.



Both the plates are insulated by silk threads. The fixed wire, *d*, with which the raised plate comes into contact, leads to a Dellman electrometer. The connections are arranged for determining the "electroscopic tension" on the poles of an open battery, to see if it is the same as the E.M.F. (See Pogg. *Ann.* 1848, vol. lxxv. pp. 88 and 220.)

This apparatus he also used to measure the Volta effect between two metals, his classical memoir on the subject being in Poggendorff's *Annalen*, 1851, vol. lxxxii. p. 1. Later he improved the condenser, bringing it into the form shown in the following figure.

Fig. 2.—Kohlrausch's Later Form of Condenser.
(See Pogg. *Ann.* 1853, vol. lxxxviii. p. 464.)



Wherever electrostatic methods were employed, and where the electroscope was the instrument of research, contact theorists had it all their own way; and it was only by apparent effort and twisting of experiments that chemical theorists could maintain their ground. But when electric currents were dealt with, and the galvanometer used, then the chemists had their turn, and they showed most conclusively that no mere contact could maintain a current unless heat disappeared or chemical action occurred: a fact obvious enough to us to-day on the principles so laboriously and finally established by Joule. By means of the galvanometer, the contact theory was so belaboured by Faraday that it ultimately seemed to give up the ghost, and the chemical doctrines triumphed. So much so, that Volta's original fact, in spite of the evidence which had been accumulated, was again doubted; and one finds in text-books culled from this period statements that Volta must have had wet fingers, or that he rubbed the plates together, or that there was moisture in the air. Also hints are given that films existed on the plate, that squeezed coats of varnish or lacquer might produce some electricity, and so on. It was pointed out, moreover, by De la Rive*, how minute a trace of chemical action could produce how much electricity, and how little electricity could affect an electroscope. But it is to be noted that any chemical action caused by damp on the plates, or moisture in the air, would be of the nature of local action, and local action is not a satisfactory producer of manageable electricity. Sir Humphry Davy is very clear on this head. He shows that chemical action need produce no electricity, instancing the burning of iron nitre on charcoal, potash and acid in a crucible or an electroscope, &c.; a plate of zinc placed on mercury and separated is found positive, but if left long enough to amalgamate, the compound shows no signs of electricity. Davy's views are singularly advanced, and are worth quoting†.

* De la Rive: *Traité d'électricité*, ii. p. 776; *Annales de Chimie*, xxxix. p. 311 (1828).

† Davy, Bakerian Lecture, 1806 (see Phil. Trans. 1807, p. 39):—"As the chemical attraction between two bodies seems to be destroyed by giving one of them an electrical state different from that which it naturally possesses . . . so it may be increased by exalting its natural energy. Thus, while zinc is incapable of combining with oxygen when negatively electrified in the circuit even by a feeble power, silver easily unites to it when positively electrified. . . . Among the substances that combine chemically, all those, the electrical energies of which are well known, exhibit opposite electrical states. . . . In the present state of our knowledge it would be useless to attempt to speculate on the remote cause of the electrical energy, or the reason why different bodies after being brought into contact should be found differently electrified; its relation to chemical affinity is, however, sufficiently evident. May it not be identical with

Quite detached from any connection with the controversy, because at that time quite unintelligible to all but one or two here and there, two papers appeared in 1851 by the President of this Section, which were the triumph and apotheosis of the chemical theory of the source of the current in the voltaic cell*. In one of these papers (that on "Electrolysis") it is irrefutably established, on the basis of the conservation of

it, and an essential property of matter?"—Page 44: "The great tendency of the attraction of the different chemical agents by the positive and negative surfaces in the Voltaic apparatus seems to be to restore the electrical equilibrium. . . . The electrical energies of the metals with regard to each other or the substance dissolved in the water, seems to be the cause that disturbs the equilibrium, and the chemical changes the cause that tends to restore the equilibrium; and the phenomena most probably depend on their joint agency." He then gives a very Voltaic account of the action of the pile—much in agreement with Sir Wm. Thomson; and endeavours to reconcile chemical and contact theorists by pointing out how essential a part chemical action plays in the production of a *current*, a most clear-sighted thing to do at that date. One more sentence may be quoted from this remarkable paper, though it is not quite so striking as the preceding. —Page 49: "These ideas are evidently directly in contradiction to the opinions advanced by Fabroni, and which in the early stage of the investigation appeared extremely probable, viz. that chemical changes are the *primary* cause of the phenomena of Galvanism. Before the experiments of M. Volta on the electricity excited by mere contact of metals were published, I had to a certain extent adopted this opinion; but the new fact immediately proved that another power must necessarily be concerned, for it was not possible to refer the electricity exhibited by the opposition of metallic surfaces to any chemical alterations, particularly as the effect is more distinct in a dry atmosphere, in which even the most oxidizable metals do not change, than in a moist one, in which many metals undergo oxidation."

* Sir W. Thomson:—1. "On the Mechanical Theory of Electrolysis and the Applications of the Principle of Mechanical Effect to the Measurement of Electromotive Forces in Absolute Units," *Phil. Mag.* December 1858. Reprint of Mathematical and Physical Papers, vol. i. pp. 472 and 490. 2. "On the Dynamical Theory of Heat, part vi. Thermoelectric Currents:" *Proc. R. S. Edin.* Dec. 1851; *Trans. R. S. Edin.* 1854; *Math. and Phys. Papers*, vol. i. pp. 232 and 316.

Helmholtz also clearly applied the conservation of energy to Voltaic circuits in his memoir *Die Erhaltung der Kraft*, read before the Physical Society of Berlin, 23 July, 1847. In this powerful memoir Prof. Helmholtz sails placidly through a great part of physics, applying to various phenomena the then new principle of the conservation of energy. He regards all action as occurring at a distance, and shows, as is well known, that, on this hypothesis, central forces are the necessary and sufficient condition of conserved energy. This part may now be regarded as superseded; but in the more special portions, among other things, he develops the mechanical theory of the E.M.F. of voltaic cells, of thermoelectric piles, and of magneto-machines; anticipating in many respects the somewhat later though independent work of Sir W. Thomson on these subjects.

Prof. Helmholtz's memoir is easily accessible through a translation, by J[ohn] T[yndall], which appeared, in May 1853, in the "new series" of 'Scientific Memoirs,' issued by Taylor and Francis.

energy, that, making exception of such irreversible effects as are not readily brought into calculation, and allowing for certain possible reversible effects to be investigated thermoelectrically, the E.M.F. of a cell is not only dependent on the chemical action going on, but is calculable numerically in absolute measure on purely chemical data supplied provisionally by Dr. Andrews. It is proper to say, however, that this brilliant theory is avowedly based on the laborious and acute experimental work of Joule on the conservation of energy in the voltaic circuit*.

In the other of the two papers (that on "Thermo-electricity") it was shown that, from the fact that a current absorbed or generated heat at a metallic junction, an E.M.F. was necessarily situated there—in other words, that the Peltier effect necessitated the previously discovered Seebeck one.

The establishment of the conservation of energy, by Joule, for ever placed beyond doubt the fact that the energy of the electric current produced by a battery was due to, and was the equivalent of, the chemical actions going on there; but it was supposed, and is still supposed (though, as I venture to think, quite erroneously), to leave untouched the question as to the precise seat of the E.M.F. in a battery.

However that may be, the success of the chemical theory of the electric current naturally caused it to be still more certainly assumed that the apparent contact-force of Volta could also be accounted for by accidental chemical action, and that without some chemical action somewhere no Volta effect could be produced. This, also, I believe to be quite false; provided always that the phrase "chemical action" be used in its ordinary sense as meaning combination, and that the word "action" be not explained away as meaning anything whatever.

2. The triumph of the chemical theorists with regard to the Volta effect was, however, shortlived, for, from 1860, the invention of the quadrant electrometer put into the hands of electrostatic experimenters a far more refined and delicate instrument than could have been thought possible a few years before; and the illustrious inventor of that instrument himself for ever put the truth of Volta's phenomenon beyond doubt, by the most simple and beautiful device of suspending a charged torsion-arm over a zinc-copper junction. By comparing the deflection so produced with that caused by a

* Joule:—"On Heat evolved in Metals and during Electrolysis," *Phil. Mag.* [3] xix. p. 260 (1841); "On the Electric Origin of the Heat of Combustion," *ibid.* xx. p. 98, and xxii. p. 204; "On the Heat disengaged in Chemical Combination," *Phil. Mag.* [4] iii. p. 481. See also Reprint of Joule's papers by the Physical Society of London (Taylor & Francis).

Daniell cell, an absolute measure of the so-called contact-force was made; and it was shown that, uniting the copper and zinc by a drop of water, instead of by a metal, no deflection was produced. It was also shown that the deflection was greatest when the zinc was clean and the copper oxidized*.

But Sir William Thomson went further than this: he sounded a theoretic note, and in a sentence revived the whole controversy about the seat of power in the pile. The sentence is this:—"For nearly two years I have felt quite sure that the proper explanation of voltaic action in the common voltaic arrangement is something very near Volta's, which fell into discredit because Volta or his followers neglected the principle of the conservation of force. I now think it quite certain that two metals dipped into one electrolytic liquid will (when polarization is done away with) be at the same potential." And then he goes on to one of those brilliant and extraordinary speculations characteristic of no one else, and applies this apparent contact-force to determine a lower limit to the size of atoms—an application obviously of transcendent interest, and of more importance than all the previous outcome of contact discussions put together †.

The whole subject now acquired a fresh interest, and the new series of experimental determinations of contact-force began.

3. Hankel's and Gerland's measurements belong to this period in point of date (1861-69), though in method and motive they probably are the outcome of the earlier period‡. Hankel uses a modified Kohlrausch method and a Bohnenberger or

* Proc. Lit. and Phil. Soc. Manchester. Letter from Prof. W. Thomson to the president, Dr. Joule, Jan. 21, 1862: "New Proof of Contact-Electricity." See reprint of papers on Electrostatics and Magnetism, p. 317.

† "There cannot be a doubt that the whole theory is simply chemical action at a distance. Zinc and copper connected by a metal wire attract each other from any distance, so do platinum plates coated with oxygen and hydrogen respectively. I can now tell the amount of the force, and calculate how great a proportion of chemical affinity is used up electrolytically before two such disks come within any specified small distance down to a limit within which molecular heterogeneousness becomes sensible. This of course gives a definite limit for the size of atoms."—*Letter to Dr. Joule, 1862, cited above.* See also Thomson and Tait, *Nat. Phil.*, Part II. Appendix F.

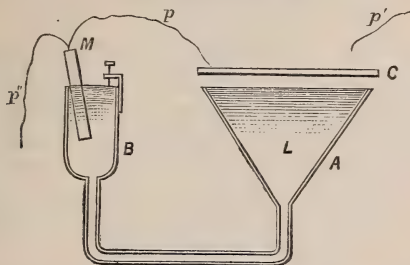
‡ Hankel, *Electr. Untersuchungen, Abh. der königl. Sächs. Gesellschaft, math.-phys. Klasse*, 1861 and 1865. See also Pogg. *Ann.* cxv. p. 57, and cxxvi. pp. 286 and 440; cxxxi. p. 607.

Gerland, "On the E.M.F. between Water and some Metals," Pogg. *Ann.* cxxxiii. 1868, p. 513; cxxxvii. 1869, p. 552. In his second paper, to get over the air effect, Gerland joins two metals through a galvanometer, and then dipping them into a liquid, observes the first swing of the needle. He also compensates the E.M.F. by Poggendorff's method. He thus determines the value of

$$M/M' + M'/\text{Liquid} + \text{Liquid}, M.$$

Hankel electroscope ; his special merit is the determination of metal-liquid contact-forces without introducing blotting paper, glass, or fingers, as the earlier experimenters had done.

Fig. 3.—Hankel's Arrangement for observing the Volta Effect between a Metal and a Liquid.



The liquid, L, is in a funnel-tube, A, B; C is a copper plate, and M the metal under observation. First, p touches C for an instant, then C is raised and made to touch p' , which leads to a Hankel electrometer.

This gives $Cu/M + M/L = k\alpha$.

Then the liquid is run out of the funnel, a plate of the metal M is placed on its mouth, and the experiment repeated.

This gives $Cu/M = k\beta$.

To eliminate k substitute a plate of zinc for M, and get

$$Cu/Zn = k\gamma.$$

$$\text{Then, finally, } M/L = \frac{\alpha - \beta}{\gamma} Cu/Zn.$$

In all these expressions air contact-forces are, as usual, neglected. But it is very tempting to try if, by increasing the number of such equations, one cannot calculate some metal/air contact-forces. Thus the special case when M is copper gives one more equation. We can then take zinc instead of M, and can also make the condenser-plate of zinc instead of copper, and so on; but we get no forwarder, fresh unknowns appear as fast as additional equations, and some of the equations are liable to degenerate into identities.

So far back as 1824 Becquerel* attempted the investigation of metal-liquid contacts; and Buff† made some measurements in 1842, but his results are scarcely likely to be reliable considering the poor experimental resources of that date.

Professor Clifton employed Kohlrausch's method in 1877, using a Thomson electrometer and a Clark cell as standard of E.M.F. He has only published a preliminary paper‡, in which he overlooks minutiae such as change of contact-

* Becquerel, *Ann. de Chimie*, 1824. He put the liquid in a copper capsule on the plate of an electroscope, and connected it with the condenser plate by his fingers.

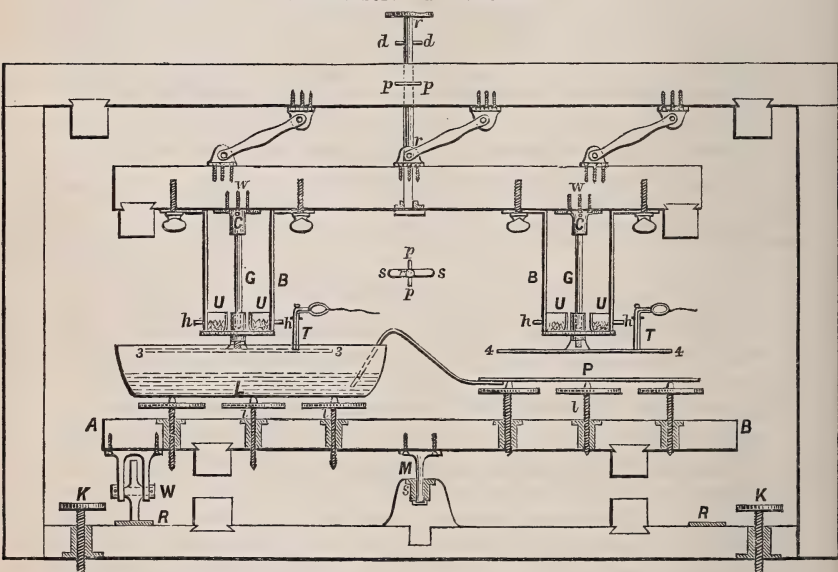
† Buff, Liebig's *Ann. Chem. u. Pharm.* 1842. He made the lower plate of his condenser the metal to be examined; on it he placed glass, and then filter paper soaked in the liquid, which he connected with the metal plate by a wire of the same metal.

‡ Clifton, *Proc. Royal Soc.* xxvi. p. 299 (1877).

force by time and adventitious circumstances. When he touches on theory he agrees with Thomson. Professor Clifton has examined the Volta effects for the substances ordinarily used in batteries with great care, and has probably elicited the maximum of accuracy possible to his method. He gives the E.M.F. of numerous virgin cells in which no current has circulated.

Ayrton and Perry in 1876 devised in Japan a very ingenious but somewhat unwieldy modification of Kohlrausch's method, and with the help of students carried out a most extensive and laborious series of determinations of metal/metal, metal/liquid, and liquid/liquid contacts*.

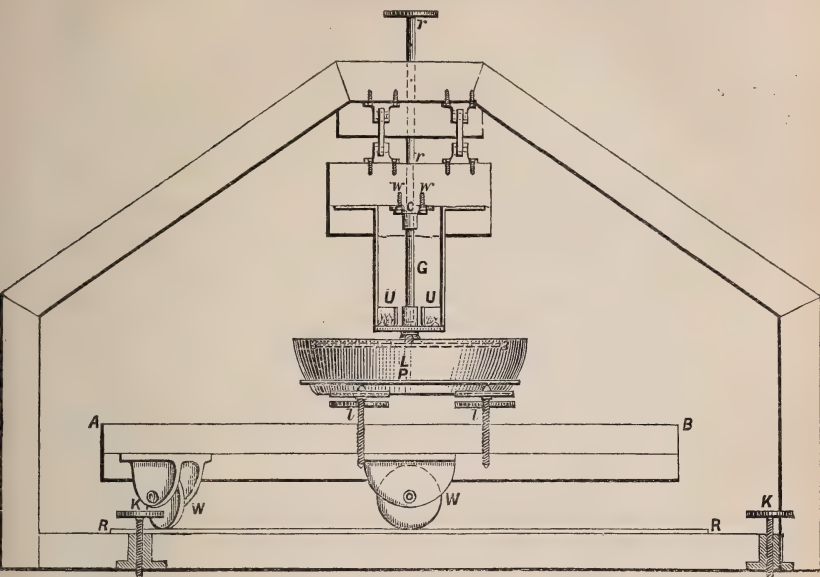
Fig. 4.—Ayrton and Perry's Apparatus for measuring the Volta effect with all sorts of substances.



The substances are arranged on the lower platform, as, for instance, the metal and liquid shown in the figure at P and L. The platform, A B, is capable of rotation through 180° on its railway, R. 3 and 4 are carefully insulated gilt plates fixed to a bar which can be raised and lowered. The experiment consists in lowering these plates close to the surfaces to be tested, and connecting them with each other for a short time; then raise them, rotate the platform through 180°, lower again, and connect them with a quadrant-electrometer.

* Ayrton and Perry, Brit. Assoc. Glasgow, 1876. No abstract printed. Part I., Proc. Royal Soc. 1877 or 1878, is a preliminary account. Part II. describes a metallic voltaic cell of magnesium and platinum and mercury, also some experiments on electrolytes of high resistance. Part III., Phil. Trans. 1880, is the complete account of their published electroscopic experiments.

Fig. 5.—End view of Ayrton and Perry's Apparatus.



On the appearance of Clifton's paper the year after, they issued a strongly worded claim* in respect both of priority and completeness—a claim which seems to me well established, for their results are the most comprehensive yet obtained, and the energy needed to devise, construct, and use such an apparatus as the one they depict must have been immense. A convenient summary of their numbers is to be found in Everett's 'Units,' second edition. The main result achieved by them is the experimental establishment of the summation law for all substances (this is not to be confused with Volta's summation or series law, which is only applicable to metals), viz. that the total E.M.F. of a closed circuit of any number of substances may be reckoned by adding up the Volta forces observed electrostatically for every pair of substances in contact. This law is, it seems to me, for reasons given later (§. 7), very probable theoretically; but still it was quite essential to have it experimentally established, especially as Ayrton and Perry point out that it is often called in question without good ground. The establishment of this law is, I say, perhaps their main work in this matter, besides the

* Ayrton and Perry: Letter published in 1877 by Meiklejohn (Yokohama).

observation of the Volta effect for various difficult substances, especially liquids and liquids.

Clifton arrives at the same conclusion with regard to summation, and gives handy diagrams, reproduced in Jenkins's 'Electricity,' of the contact-force at the different junctions. My own opinion is that the intended and obvious significance of these diagrams is theoretically wrong, but they embody certain experimental results conveniently, and they can be interpreted properly.

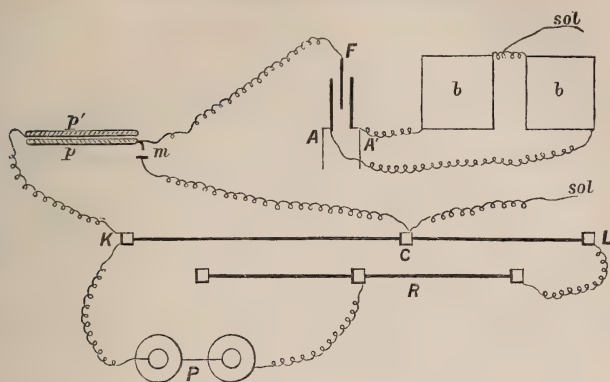
Both Clifton and Ayrton and Perry appear to believe in the great constancy of the value Zn/Cu . Clifton gives it as .8516 volt ("Quelle précision!" somewhat sarcastically ejaculates Pellat, who himself finds it to vary between .63 and .92). Ayrton and Perry assert that it is more constant than a Daniell. I believe that both Professor Clifton and Professors Ayrton and Perry have made several experiments besides those recorded in their communications to the Royal Society; but as they have not been published, I can give no account of them.

Among the Thèses presented to the Faculty of Science in Paris in 1881 we find an important memoir by Pellat*, which reviews the whole position very clearly, and records a series of determinations of Volta force among metals, determinations which are evidently the most accurate and satisfactory yet made. He adopts the capital experimental method of neutralizing the charge of a condenser by a Pogendorff or compensation method, and thus converts Kohlrausch's into a null method, for which a very sensitive electroscope is all that is needed. The plates of the two metals set face to face are connected, not directly, but by a greater or less length of a graduated wire conveying a current; and the position of the slider on the wire is adjusted by continually separating the plates and testing until no charge at all is found. The step of potential on the wire is then precisely equal to the "contact-force" between the plates; for this would have caused a charge in a similar but uncompensated condenser, and the step of potential on the wire has neutralized it.

Compensation methods of a sort had been used before by

* Thèses présentées à la Faculté des Sciences de Paris, pour obtenir le Grade de Docteur-ès-Sciences physiques, par M. H. Pellat, Professeur de Physique au Lycée Louis le Grand, No. 461; Juin 22, 1881. See also *Journal de Physique*, 1881, xvi. p. 68, and May 1880: "Différence de potential des couches électriques qui recouvrent deux métaux en contact."

Fig. 6.—Diagram of Pellat's Method.



Légende.

KL, compensateur.

C, curseur.

R, rhéostat.

P, piles Daniell fournissant le courant du compensateur.

 $p'p$, plateaux du condensateur (p fixe, p' mobile). m , interrupteur.

F, feuille d'or.

A et A', plateaux attractifs de l'électroscope.

 bb , batteries de 100 volts chacune pour charger les plateaux A A'.

The diagram, fig. 6, pretty well explains itself. The contact at m is broken the instant before p' is raised.

Fig. 7.—Pellat's Condenser.

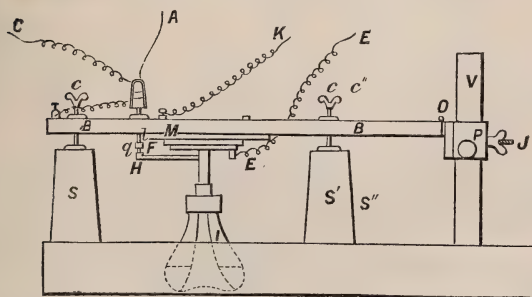


Fig. 7 shows how this was done in practice. Pulling the string, A, raises a sliding-pin with a shoulder, q , fixed to it a millimetre below the bar, B. The pin thus first breaks contact near H, and then raises the hinged bar BB, to which the upper plate, M, is attached. The upper plate, being always in connection with a battery, requires no special insulation. The lower plate is carefully supported by a Thomson insulator, I. The lower plate was considerably smaller than the upper one, and it was further protected from stray inductions by a guard-screen. Screws, c , served to regulate the distance between the plates.

Gerland and by Thomson. Gerland applied compensation to determine the E.M.F. of two metals dipped into a liquid, and Thomson applied it to the divided-ring experiment, thus making it very analogous to that of Pellat*.

Pellat also adopts Sir William Thomson's view that the Volta effect is due to a true contact-force between the metals, and that it represents a real difference of potential between them when in contact; at the same time he is careful to point out that no rigorous proof can be given of this, and that all that is really and certainly measured electrostatically is the difference of potential between what he calls the electric coats (*les couches électriques*), or what may be more simply called the air-films, on the two touching metals.

The following is a summary of some of Pellat's measurements, gold being the metal with which all are compared, and the numbers being given in volts.

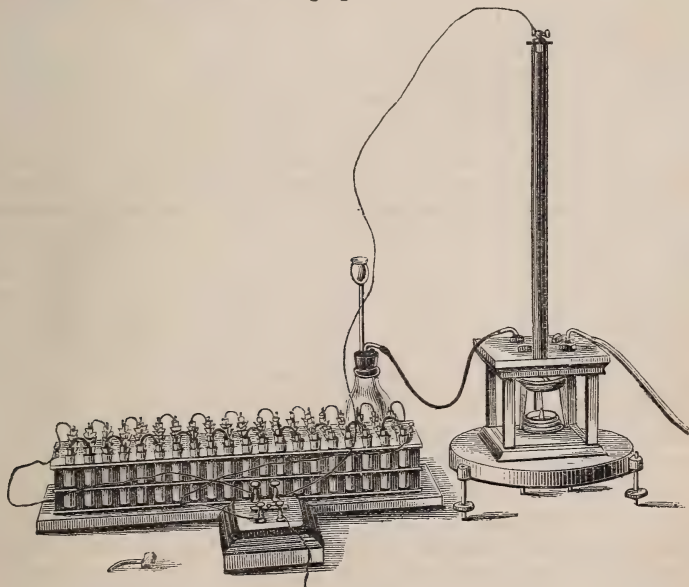
“Difference of Potential of the Electric Coats which cover a Metal and Standard Gold metallically connected, and both in Air.”

Metals used.	E.M.F. in volts.		Metals used.	E.M.F. in volts.	
	With very clean but scarcely scratched surface.	With surface strongly scratched by rubbing with emery.		With very clean but scarcely scratched surface.	With surface strongly scratched by rubbing with emery, or in some of the last cases with cloth or filter-paper.
Zinc	·85	1·08	Iron	·29	·38
Lead	·70	·77	Brass	·29	·37
Tin	·60	·73	Copper	·14	·22
Antimony ...	·44	·46	Platinum ...	—·03	+·06
Nickel	·38	·45	Gold	—·04	+·07
Bismuth.....	·36	·48	Silver	—·06	+·04
Steel	·29	·44			

* I find indeed that Sir W. Thomson completely anticipated Pellat in the application of galvanic compensation to the measurement of Volta effects; for in ‘Nature,’ April 14, 1881 (vol. xxiii. p. 567), is printed an account, given in brief at the Swansea meeting of the British Association (see Trans. of Sects. 1880, p. 494), which relates how the divided-ring experiment naturally developed into more complete compensation with slide resistances, and that an extensive series of measurements were made on this plan in the years 1859–61 with results quite in agreement with those published by Hankel in 1862. Other experiments were made since 1861 with results confirmatory of those of Pfaff, 1821, showing the Volta effect to be independent of the surrounding gas. The description of all these experiments was therefore withheld till something new should be obtained by the method, and was not published until Pellat's paper had

4. Meanwhile some experimenters, starting with a belief in the chemical origin of the Volta effect, had made experiments supposed to support this view. Mr. J. Brown, of Belfast, in 1878, repeated Thomson's divided-ring experiment*, as well as Kohlrausch's condenser-experiment, in other gases than air; and found a very decided difference, and even a reversal of sign, when sulphuretted hydrogen was substituted for air. The metals Brown used were copper and iron, and he obtained a 1-centimetre deflection in the direction indicating iron + in air; while in SH_2 he obtained a 3-centimetre deflection, indicating that iron was -. On readmitting air, the deflection again reversed, and so on until the copper coated itself with a blue film of sulphide, when the deflection became undecided, owing, as Brown supposes, to "the cessation of chemical action."

Fig. 8.—Mr. J. Brown's Arrangement for observing the Volta Effect in different Gases by Sir William Thomson's Method of a bimetallic ring with an electrified needle hanging over it.



appeared in the *Journal de Physique*, May 1880. Fig. 10 sufficiently exhibits Sir William's arrangement. In a postscript are described a few additional experiments of the same kind as those published in 1881 by Schulze-Berge, in which a platinum plate is soaked for a certain time in dry hydrogen or oxygen, and then used in the Volta condenser. The observation is made that merely soaking a plate in gas is more effective than electroplating it with the same gas with an E.M.F. of a volt.

* J. Brown, *Phil. Mag.* August 1878, Feb. 1879, and March 1881; see also *Brit. Assoc. Trans. of Sect.* 1881, and 'Electrician,' vol. vii. p. 165.

In 1881 he observed a time-change (decrease) of the Volta effect at a copper-zinc junction, and reckoned that at the first instant after cleaning the potential-difference would be as high as .9 Daniell, "which," he says, "agrees with J. Thomsen's determination of the difference of the heats of combustion of zinc and copper and oxygen." He here gives a hint of holding a heterodox notion which I do not find in any other of his writings, and which I believe he abandoned, even if he ever really held it.

In 1879 Brown tried a copper-nickel divided ring, substituting HCl for air, and here also succeeded in obtaining a reversal of sign. He also arranged a divided ring of wet blotting-paper, and showed that there was a difference of potential when the two halves were touched with a zinc-copper couple (which is not remarkable); but he then goes on to draw a moral, and to say that the slit of the divided ring corresponds to the air-film, and the wet paper to the moisture-film in the ordinary Volta-condenser experiment. The film of moisture on the zinc plate is thus shown to have a + charge, and that on the copper a negative. If it be objected that the better the plates fit, the better the manifestation of contact E, it is to be replied that it is not to be supposed that there is no air between them anyhow (says Brown). Probably, he says, gas produces the difference of potential only so far as it forms a film on the surface. When a metal and a liquid are experimented on, it is probably really a two-fluid cell, the other fluid being that condensed on the surface of the metal.

Brown thus goes strongly for the activity of the films, or condensed air-sheets, which certainly exist on the surface of solids, and which may play an important part in the matter; but he supposes that these films act by corroding or attacking the plates, and that such a film is necessarily existent between surfaces nominally in contact if any Volta effect is to be produced; so that if the metal faces really and truly touched all over, they would show no charge when separated. Moreover, he lays it down that the potential difference is only observed while chemical action is going on; but that so soon as it ceases, from any cause, at once the Volta effect ceases too. In all this I entirely differ from him; but his experiments are very interesting and much to the point.

They cannot, however, be regarded as settling the question—the very important and fundamental question—as to whether the Volta effect depends on the atmosphere or medium surrounding the plates, or whether it is an absolute effect depending on contact alone. Experiments on this point are absolutely discordant; and it seems to be one of those points

which it is very difficult to settle by direct experiment. For if by using a chemically active gas instead of air, you get a positive result or change in the Volta effect, the answer from the other side is: "Yes, of course, because your plates are corroded and coated with sulphide or chloride, or something whose contact-forces come in and modify everything." If, on the other hand, you get a negative result when you substitute some inert gas like hydrogen for air, then it is objected that you have not removed the air-film which the plates had contracted from long standing in the air; and if you answer indignantly that you did, and that your hydrogen was perfectly pure, it is replied with a sneer, "Oh yes, it is not so easy to get pure hydrogen as you seem to think."

Moreover, suppose a positive effect on changing the gas *was* established; what then? Nothing is settled except that the metal/air contact-force is proved to be somewhat different from the metal/gas contact-force. There seems to be really no way of knocking contact-force on the head experimentally, and this probably because it is a reality: *there really is* a contact-force at every junction of dissimilar substances; and the E.M.F. of a circuit, whether it be inductive or conductive, is always the sum of such contact-forces. I do not say that the contact-force at any given locality has the value ordinarily assigned to it as the result of experiment.

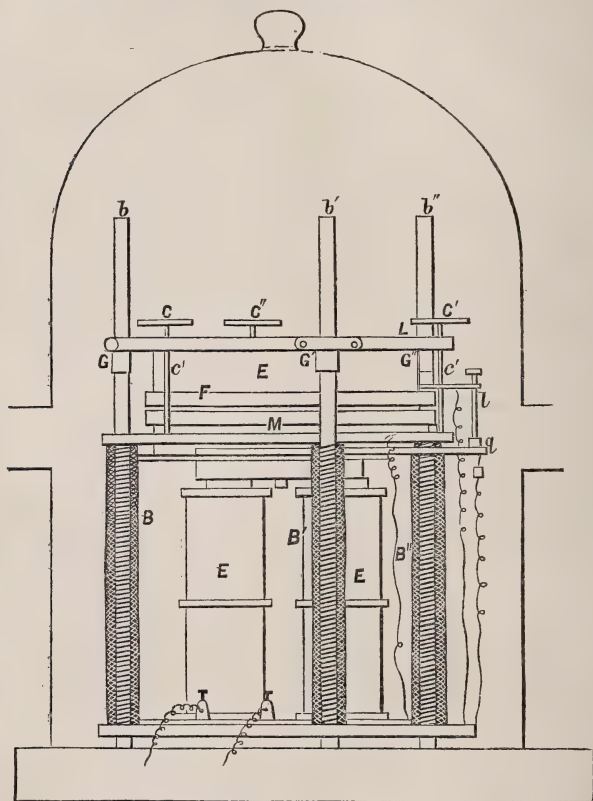
The earliest attempt made to examine the question as to whether the Volta effect depended on the atmosphere was made by Pfaff* in 1829, who used dry and damp air, oxygen, nitrogen, hydrogen, carburetted hydrogen, and carbonic acid, and he found that there was no difference so long as no visible chemical action occurred; but it must be noted that the opposing faces of his plates were varnished. De la Rive, on the other hand, asserted that there was no Volta effect in the slightly rarefied air then known as "vacuum."

In recent times Pellat has investigated the subject, and has come to a conclusion in agreement with Pfaff, viz. that the differences are very small. The metals used by Pellat were copper and zinc, and the gases were air dry and damp, dry oxygen, dry nitrogen, dry and pure hydrogen, dry and pure carbonic acid. He finds slight variations, but exceedingly slight, and such as Pfaff, Exner, and Brown could hardly have detected. He says:—"Au surplus il est fort probable que, si quelques-uns des auteurs précédents avaient tenté les expériences que j'ai faites au sujet des gaz, ils auraient trouvé des résultats négatifs; les faibles variations produites par le changement des propriétés du gaz que j'ai pu mettre nette-

* Pfaff, *Ann. de Chim.* 2 ser. xli. p. 236. The metals he employed were copper, tin, and zinc.

ment en évidence, grâce à la précision des mesures, sont au-dessous des erreurs expérimentales de leurs méthodes, ou à peine supérieures dans les cas les plus favorables.”

Fig. 9.—Pellat's Apparatus for experimenting in different Gases and at different Pressures.



The movable plate is now the lower one, and it is pulled down by an electromagnet, E, a little way against the springs, B, which tend to drive it up against the screw-tops, C. Contact is automatically broken at *q* the instant before separation. The bell-jar has 35 litres capacity, the diameter of each plate being 15 centimetres. It must be impossible to employ anything like pure gases in a bell-jar enclosing such a bulky mass of heterogeneous material; and the pressure was found not to go below 2 or 3 centimetres of mercury. However, he has since made a smaller arrangement of 1 litre capacity, with plates 9 centimetres diameter, and, what is more important, with the electromagnet outside, and nothing inside but glass, mica, and metal. In this the pressure goes down to a millimetre. But even this is not all that could be wished. Moreover the experiments described had been made with the larger apparatus.

In all the above gases he has also studied the effect of varying the pressure. Lowering the pressure slightly *increases* the observed difference of potential; but the change lags a little behind the pressure-variation. Damp and dry air behave in the same way. In oxygen the effects of pressure are rather better marked. Nitrogen gives nearly the same numbers as air; but after it has been in for some time, the numbers are slightly lower than at first. Hydrogen gives a little greater effect than even rarefied air; rarefying hydrogen does not alter it much. Carbonic acid gives the same numbers as rare air or dense hydrogen. As for liquids, plates wetted with alcohol give the same result as if immersed in plain air.

Von Zahn* also tried a condenser in various gases, and found no difference; but when he tried a platinum-zinc condenser in the highly rarefied air now known as vacuum with some melted sodium in a branch tube to absorb all the oxygen, the Volta effect was diminished, and only represented a potential difference of half a Daniell. I am not clear whether sodium can be trusted to ultimately absorb every trace of oxygen; but I should judge it would take a very long time: and as to rarefaction, dividing the numbers of molecules in a vessel by a million or two leaves them quite numerous enough to accomplish anything they want.

Sir W. Thomson has also made experiments in different gases with negative results†. These experiments are not described in detail, but they were made with the apparatus shown in fig. 10.

The views of Ayrton and Perry on the subject of the effects of atmosphere underwent modification between their first paper and their third. In their first paper they say they have good reason to believe that there is no great difference of potential between a metallic or liquid surface and the air in contact with it

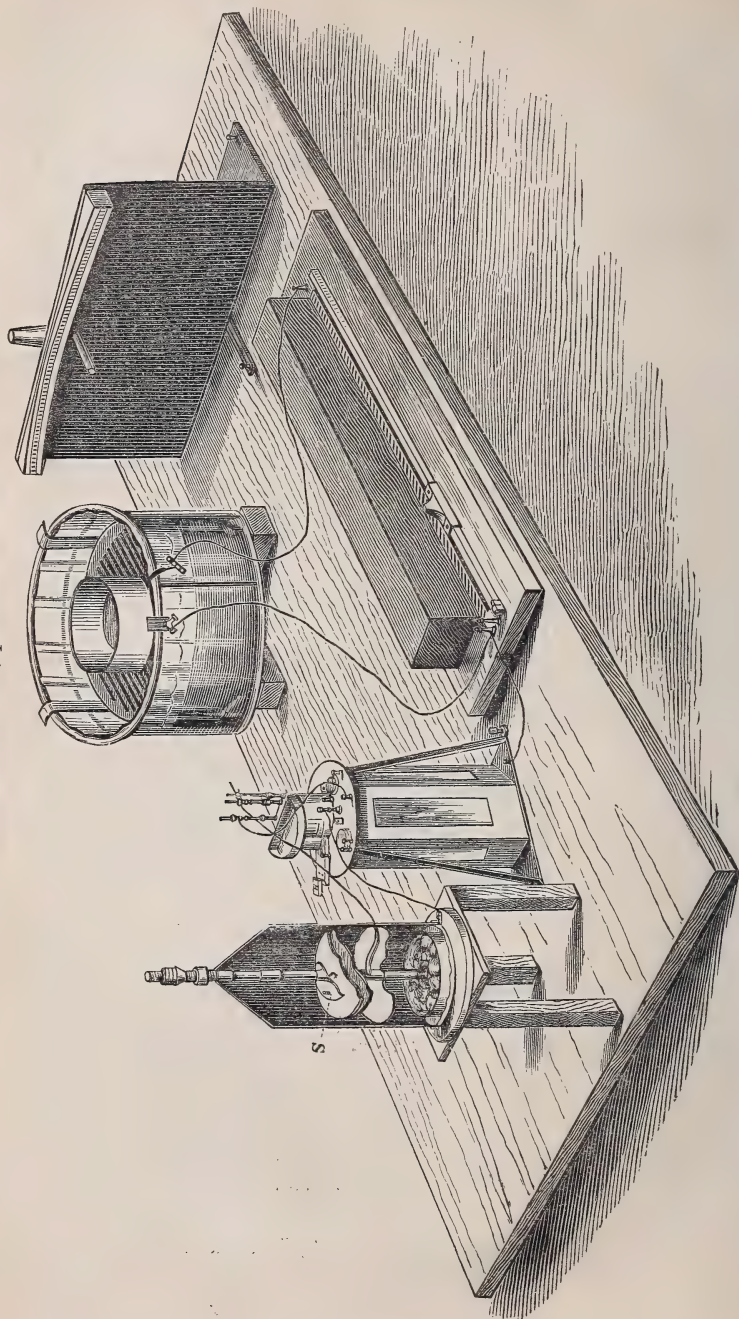
Clerk Maxwell, in a letter to the 'Electrician,'‡ pokes fun at them for this, saying:—"A statement like this, coming from men whose scientific energy is threatening to displace the centre of electrical development, and to carry it quite out of Europe and America to a point much nearer Japan, is worthy of all attention, even without an explicit statement of their 'good reason.' But Mr. J. Brown has shown (Phil. Mag. August 1878) by the divided-ring method of Sir W. Thomson, that whereas copper is negative with respect to iron in air, it is positive with respect to iron in H_2S . It would

* Memoir quoted below.

† Thomson, Brit. Assoc., Trans. of Sect., Swansea (1880), p. 494.

‡ 'Electrician,' April 26, 1879.

Fig. 10.—Sir William Thomson's Compensation arrangement for measuring Volta-force in Air and Gases by a null method, devised and used in 1859, published in 1880.



The upper plate of the condenser is uninsulated, and slides up and down from one fiftieth to a quarter or half an inch from the lower insulated plate, which is permanently joined to the electrometer. The upper plate is put in communication with the lower one momentarily through the slide resistance, and is then raised, the position of the slide being adjusted till no deflection ensues.

appear, therefore, that the reason why the results of metals by the ordinary 'contact-force' experiments harmonize so well with the comparison by dipping both metals in water or an oxidizing electrolyte, is not because the E.M.F. between a metal and a gas or an electrolyte is small, but because the properties of air agree to a certain extent with those of ordinary electrolytes. For if the active component of the electrolyte is sulphur, the results are quite different; and the same kind of difference occurs when hydrogen sulphide is substituted for air."

In their third paper*, therefore, we find Ayrton and Perry's views changed, and they clearly state that their experiments, like all those based on inductive methods, leave the question of air-contacts quite undecided.

They then go on to say:—"One way of determining the E.M.F. of contact in volts between a substance and air, and a way we hope shortly to employ, is to repeat all these exact contact-experiments in different gases [it is not quite true that this would give the results required, because it would only give differential effects; very important to observe, no doubt, but not the same as observing the actual contact-force of air or of gas]. . . . We shall thus ascertain whether the contact-difference of potentials of a substance and a gas differs much for different gases. Qualitative experiments in this direction have already been made with very interesting results by Mr. Brown of Belfast, but his experiments differ from ours in not being quantitative." [Only, as their experiments do not yet exist, Mr. Brown's have still some value.]

In vol. xii. of Wiedemann's *Annalen*† I find some interesting experiments by Schulze-Berge on contact-force between metals and gases. He uses a condenser and different gases, but the plates of his condenser are both of the same metal, and he coats one of the plates with a film of the gas, say chlorine, or hydrogen, or ozone, and leaves the other covered with air. To measure the potential-difference he employs the compensation method of Pellat, and his arrangement seems fairly satisfactory. But he does not explicitly enter on the question as to the seat of E.M.F. in the Volta experiment (except in a controversy with Professor Exner). He assumes that a contact-force between metals and metals and between metals and liquids has been established, and he wishes to extend it to the contact of a metal and a gas.

* Phil. Trans. 1880.

† Schulze-Berge, "Ueber die Elektricitätserregung beim Contact von Metallen und Gasen," *Ann. der Phys. u. Chem.* xii. p. 293 (1881).

Believing firmly in the existence of films of condensed gas at the surface of a solid, which films require time for their formation or removal, he deems it sufficient to soak one of the plates of his condenser in the gas to be examined, and then to take it out and measure the difference of potential between it and the other plate coated only with a film of air. Tried thus, ozone rendered gold, platinum, and brass negative as against the corresponding air-covered plate. Hydrogen rendered its platinum strongly positive ; but its influence on gold was slight, and on brass uncertain. Chlorine and bromine made platinum negative, and ammonia made brass positive.

It may be readily objected that what the soaking with gas accomplished was not the formation of a film of gas, but a film of actual chloride, oxide, or other combination. Against this is to be urged the fact that after removal from the gas the effect diminished with time, and the plates gradually returned to nearly their former state. He tried if he could remove the gas film from one of the plates by exhaustion under an air-pump, and the plate so treated exhibited a difference when taken out and compared with an ordinary plate ; but he was cautious enough to repeat the experiment, leaving the plate under the bell-jar for the same time and not exhausting. The same difference appeared, so he attributes it to possible grease.

This is the right sort of way to make experiments ; and if everybody experimented with proper care there would be vastly fewer papers published, and science would progress on the whole faster.

At present it feels to me overladen with a mass of publication, mostly of necessity by men of not absolutely the first order, much of it with no sort of clearness or insight, but rough, crude, and ill-digested. A man makes a number of experiments ; he does not stop to critically examine and weigh them, and deduce from them their meaning, nor indeed does he often take the trouble to examine whether any definite meaning can in their then shape be drawn from them ; but he rushes with them into print, producing a memoir of wearisome length and sometimes extreme illiterateness of style.

Some one else then has the trouble of wading through the heap to see whether any fragments of value may perchance be imbedded in it, and probably he is unable to come to much definite conclusion, because he cannot be in so good a position for criticism of the experiments as the original author was. He therefore writes a paper pointing out defects and errors in the communication. Others take up the same line,

the original man replies, and so there is a controversy, and nothing is really settled at all. Finally, some one else independently goes over the whole ground from some distinct point of view, makes a few well-planned, clear, and decisive experiments, describes them in a compact and readable form, and there results a definite gain to science. But how much better would it have been if this last paper had been the only one published! Unless a man is an experimental genius of the highest order, it is necessary for him to think for far more time than he experiments, if he wishes to advance and not to lumber his science. If it be objected, as indeed it may with great truth be, that one man's life and capacity are not sufficient for this in the present state of knowledge, the objection constitutes a strong argument in favour of the proposition that the time has come for an organization of science and a more definite division of labour.

To return to the experiments of Herr Schulze-Berge, one is not able to say after all that they are very satisfactory, for they do not distinctly settle any question. The general conclusion he draws from them is the apparently safe one that the contact-force between a metal and a gas is not in general the same as between a metal and air. Even this is not absolutely safe, however, because it might conceivably be that an air/gas contact-force caused all the difference. Granting that this is unlikely, the experiments are in favour of a contact-force between metals and air or gas; but they do not establish the fact any more strongly than, if so strongly as, Mr. Brown's experiments had already done. The weak point in both is the possible corrosion of the plates and formation of films of alloys or compounds, which may be the real source of the observed difference of potential.

And against the existence of a contact-force between metals and various gases, the experiments of Pellat and others are to be remembered, which resulted in the conclusion that a condenser made of two different metals showed nearly the same Volta effect, whether the atmosphere surrounding the plates was air or hydrogen.

If it be assumed that the experiments of Brown and Schulze-Berge establish their point, and that Pellat's apparatus for different gases (fig. 9) is satisfactory (rather a large assumption), I am unable to reconcile the discrepancy, except by suggesting that Pellat did not take sufficient pains to remove the condensed-air sheet originally on both his plates. It is of course just possible that the *difference* between the potentials of the two metals might be the same in two gases though the absolute potential of both was different, but it is improbable.

In this connection I must notice also a rather long memoir* by Dr. W. von Zahn, published in 1882, which reviews the whole subject, and describes an elaborate series of measurements made with an apparatus something like what one might suppose Ayrton and Perry's to become if it were arranged for use in different gases and *in vacuo*. He refers with admiration to Pellat's work in the preface; and I do not suppose imagines that his own numerical determinations can compare with Pellat's for accuracy where they overlap, seeing that he only makes use of a sort of combination of Kohlrausch's and Hankel's methods, with a Hankel electrometer as a measuring instrument †. He has tried, however, a large number of substances as well as ordinary metals, such as powdered antimony, iron and nickel reduced by hydrogen, many kinds of carbon, Fe_3O_4 , manganite, pyrolusite, copper oxide, lead "hyperoxide," iron glance, and other minerals. He has measured the Volta effect in various gases and at different pressures, and finds, like Pellat, that it does not appreciably vary.

He has also examined the effect of temperature on the Volta effect, though he appears to think that this ought to bear some close relation to the phenomenon of Seebeck, a natural mistake many years ago when made by Avenarius, whom it led most happily, though fortuitously, to the true, and by him experimentally verified, law of E.M.F. in a thermoelectric circuit ‡. However, Zahn finds that experiment lends no support to this view, and says that a larger series of results must be obtained before basing a theory on them. Von Zahn is a confirmed contact theorist, and he victoriously assails several experiments supposed to be distinctly in favour of a chemical view of the Volta effect. He says he publishes his results because of the extraordinary discoveries being propounded by Professor Exner (such as that a thermopile will not work in a vacuum) §,

* *Untersuchungen über Contactelektricität*, von Dr. W. v. Zahn: Leipzig, Teubner, 1882.

† A Hankel electrometer is a modification of Bohnenberger's, in which a battery with middle to earth is substituted for the dry pile; the plates on either side of the gold leaf are minutely adjustable, and the motions of the gold leaf are read by a microscope. It is sometimes preferred to a quadrant for its small capacity and dead quick motion; it can be made very sensitive, but it can hardly be a satisfactory measuring instrument. Pellat used it, but only as an electroscope.

‡ Avenarius, "Die Thermoelektricität, ihrem Ursprunge nach, als identisch mit der Contactelektricität betrachtet," *Pogg. Ann.* cxix. 1863. See also *Pogg. Ann.* cxxii., where he proceeds to calculate Volta effects from thermoelectric data.

§ I have been unable to find this extraordinary statement in Exner's works, but it is quoted again by Ayrton and Perry, *Phil. Mag.* 1881, p. 49. Exner seems to have said that the thermoelectric power of bismuth-

and because of the vague and unsatisfactory views of physicists in general on the matter (*e. g.* of Hart); but I am bound to say that, so far as I can judge, Dr. von Zahn's own experiments are not of that conclusive and decided character that one had hoped for from his start, and he signally fails to sum up the facts in a neat and crisp manner. He adheres to the contact view, but his adhesion scarcely seems to me to be based on strong evidence; and in fact his theoretical views seem a little superficial considering the date at which he writes; so that one may admit pretty well all he says about contact and not chemical action as the cause of the Volta effect without being deeply committed to any specially true or specially false position*. His best experiment, and a very crucial one if only it could be perfectly performed, is the attempt to measure the Volta effect in an absolute vacuum. A pair of zinc and platinum plates are sealed up in a brass and glass vessel in such a way that one of them is capable of moving up and down, and thus of varying its distance from the other. Gravity is employed to separate the plates, the whole vessel being inverted. The vessel is filled with dry nitrogen and exhausted for some days, occasional heat, P_2O_5 , and melted sodium, being employed to improve the vacuum.

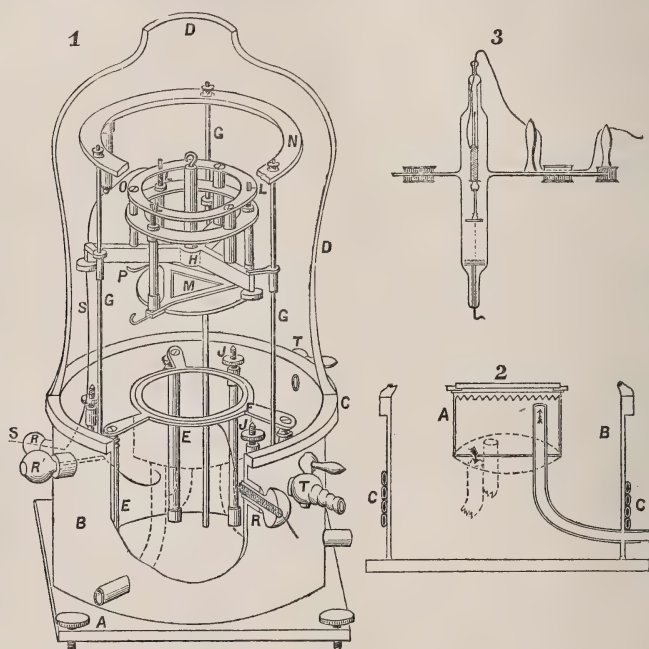
The result is that the Volta effect is very decidedly "too small," going down to half a Daniell, so far as the measurements made by his not entirely unobjectionable method can be trusted; but he does not seem to think that this is much of an argument either way, and, not finding any further change after some days, he did not pursue the investigation further by letting in some air and seeing whether the old value is restored, though he perceives clearly that this would be a crucial experiment. This abstinence is so remarkable that it

antimony is destroyed by immersing the pile in pure nitrogen; and Young of Princetown takes the trouble to examine whether it is so experimentally (see *Phil. Mag.* x. 1880, p. 450), and finds the thermoelectric power of metals the same at one millionth atmosphere as at 1. This, however, would be altogether inconclusive if experiment were needed to settle it.

* I am afraid this is not peculiar to Dr. von Zahn. It may be that the German writers on the subject are too busy accumulating facts to care much about their precise theoretical bearing; but I notice a very loose and unsatisfactory way of putting forward secondary matters as if they were the real points at issue, and of never really getting to the heart of the matter. It is singular that the four questions or heads under which that eminent writer, Professor Wiedemann, proceeds to discuss the attempts which have been made to settle the question of the seat of E.M.F. are such that if a categorical answer to each were, by supernatural means, vouchsafed to us, we should be, I believe, none the wiser (Wiedemann, *Elek.* vol. ii. p. 985, new edition).

seems necessary to quote his own grounds for it, and I do so in a footnote*.

Fig. 11.—Von Zahn's Apparatus for experimenting in different Gases at different Pressures and Temperatures, and also in high Vacua.



The upper plate, M, is attached by glass rods to a sledge, H, which slides on vertical steel rods, G, being pulled up by a string. On reaching its highest point it comes into contact with an insulated platinum wire, S, which communicates with a Hankel electrometer through one of the insulated exits, R. The bottom plate is supported on the ring, E. T is the stopcock for exhaustion and change of gas. Fig. 2 shows the appendage to the bottom plate for warming it by a current of steam. Fig. 3 shows the apparatus used for high vacua. The two plates are zinc and platinum, and the platinum is arranged to fall by its own weight when the whole thing is inverted. The diagram shows it in its highest position, and just going to fall back into its dotted position. The friction of its guide-rod seems not wholly satisfactory.

* "Andere etwas bessere Beobachtungsreihen gaben ähnliche Werthe. Bei allen war die Kleinheit von C. (the apparent Volta effect Zn/Pt) auffallend. Nun stellt ja dieser Werth, wie oben besprochen, nicht die Potentialdifferenz Zink-Platin dar, für seine Kleinheit muss aber eine anderweite Ursache vorliegen. Dieselbe kann ich zunächst nur darin finden, dass die Zinkplatte bei Anfertigung und weiterer Behandlung des Apparates sichtbar angelaufen ist (auf der einen Hälfte sogar blaue Färbung angenommen hat).

"Es liesse sich allerdings vermuthen, dass diese offenbar zu geringe

Zahn goes on to describe an experiment with bright sodium *in vacuo* instead of zinc, the sodium having been long kept melted in a laterally connected bulb before being introduced into position. He finds the sodium strongly positive to copper; but there can be nothing crucial about this experiment, I imagine, for metal in contact with glass may so easily give rise to disturbing electrifications.

I believe he must have employed the best vacuum of any experimenter on this subject, and that he has therefore gone most near to the proof of what I cannot help believing will be found to be the truth, viz. that the Volta effect in an absolute vacuum or perfectly inert gas (old air-sheets *et hoc genus omne* having been thoroughly removed) is very small. But if it be the case, as I believe it is, that the effect is almost independent of the *quantity* of oxygen present, so long as it is present, the difficulty of making the experiment so as to be sure of the absence of even the last few thousand or million oxygen molecules is almost overwhelming. The question of the dependence of Volta force on atmosphere remains thus undecided; and all the evidence which I can adduce in favour of such dependence is this incipient decrease observed by von Zahn, the little too-mixed-up observation of Mr. Hart (described later), the measurements of Schulze-Berge, and the more decided experiments of Brown. It may indeed be readily held that the weight of experimental evidence tends the other way, since most experimenters on the subject (Pellat, Schultze-Berge, von Zahn, and I may add Sir W. Thomson) have left off just as pure contact theorists as they began. I would attempt an experiment myself, save that I am so profoundly impressed with the difficulty of making one in which no fault or loophole can be found, and which will by every one be deemed satisfactory and final; so I prefer to base my views on a general survey and on fairly conclusive reasoning, rather than on a crucial but almost impossible experiment.

5. Perhaps this is now the place to refer to the somewhat erratic series of papers by Professor Franz Exner, of Vienna*.

Differenz in der wirklich wesentlichen Verminderung von Feuchtigkeit und Sauerstoff gesucht werden müsste, so dass der Apparat nach dem Oeffnen eine stärkere Spannung zeigen würde. Dies wäre dann wirklich ein *experimentum crucis* zu Gunsten der chemischen Theorie. Diese Entscheidung vorzunehmen wird aber erst dann nothwendig sein, wenn nach längerer Zeit, wo das eingeschmolzene Natrium noch mehr alle Reste von Sauerstoff beseitigt haben wird, eventuell noch Wiederhitzen und dergl. der jetzige Zustand des Apparates unverändert wieder gefunden sein wird."—*Von Zahn's Memoir*, p. 48.

* Exner, *Sitzb. der Akad. der Wissensch. Wien*: July 1878, "On the Nature of Galvanic Polarization;" July 1879, "On the Cause of the Production

He sets himself to disprove the existence of contact-force in the most straightforward and obvious manner, and to establish the fact that there is no electrical evolution without definite and actual chemical action. To this end he announces the following propositions :—(1) that two metals in a chemically indifferent medium show no electricity; (2) that the potential-difference of two connected metals in air is exactly half the difference of their heat-combustion energies; and (3) that two pieces of the same metal produce contact-electricity as soon as they are put into chemically different atmospheres.

The experiments by which he supports these assertions have, every one of them, been elaborately and severely criticised by Beetz, Hoorweg, Julius, Schulze-Berge, Von Zahn, Ayrton and Perry, Pellat, and Wiedemann; and his numerical determinations of contact-force appear to be unique*.

It is not necessary for me to enter into a discussion on the merit of his experiments, inasmuch as the mere fact of the existence of so great a body of hostile opinion is sufficient to show that they are not of a kind best qualified to produce conviction. The theoretical views which led Professor Exner to formulate his second statement above, that the potential-difference of two connected metals is equal to half the difference of their heats of combustion per equivalent, are, I am sorry to say, quite unintelligible to me. They depend on the hypothetically necessary existence of films of oxide, between which and the metal there is supposed to be a considerable difference of potential. Perhaps a few quotations from Professor Exner's first paper on contact-electricity will render his position clearer†. His views are but little really different

of Electricity by the Contact of Heterogeneous Metals;" December 1879, "On the Theory of Inconstant Galvanic Elements;" May 1880, "On the Theory of Volta's Fundamental Experiment;" July 1880, "On the Theory of Galvanic Elements;" November 1880, "On the Nature of Galvanic Polarization;" July 1882, "On some Experiments relating to Contact Theory."

* Beetz, Wiedemann's *Annalen*, xii. p. 290; Hoorweg, *ibid.* xi. p. 133 (1880), and xii. p. 90; Julius, *ibid.* xiii. pp. 276 and 296; Schulze-Berge, *ibid.* xv. p. 440, as well as xii. pp. 307 and 319; Von Zahn, p. 41 and Preface, of his Memoir; Ayrton and Perry, *Phil. Mag.* 1881, p. 43; Pellat, *Paris Thèses*, No. 461, p. 17; Wiedemann, *Elektricität*, ii. pp. 992-995.

† I quote from Mr. J. Brown's translation (*Phil. Mag.* Oct. 1880) of a paper in Wiedemann's *Annalen* of the same year, with some abbreviations :—"An investigation concerning the nature of galvanic polarization has led me to a quite distinct view of the origin of the so-called contact-electricity, a view which will be supported by experiments following. I have shown that the original cause of the polarization-current is to be sought for, not at the contact of the electrodes with ions liberated on

from those of De la Rive and other older "chemical theorists;" but they are (especially in later papers) expressed in so definite and decided a manner that they have excited a sharper controversy than vaguer and more hesitating writings could. This indeed may be regarded as their special merit. The main objection which can be taken to them relates to the quantitative statements: these are vigorously made, but they seem unwarranted by facts accumulated by all other observers, though indeed some of his own experiments certainly seem to support them. It has also been objected that he misinterprets some of his experiments.

He has got hold of the notion that the heat of combustion has some sort of relation to the Volta effect, and there I am heterodox enough to agree with him. But what the relation is, and how it acts, and what sort of potential-difference you ought to expect in accordance with theory, concerning all these things I am utterly at variance with him; and I deem it prudent not to attempt to represent views which I am unable to understand, because it is unlikely that I should do them justice.

them, but in the recombination of the latter; and the E.M.F. of the current so produced is measured by the heat-value of this combination, just as the E.M.F. of any galvanic cell is measured by the heat-value of the chemical process going on in it. With a so-called contact-action the existence of the polarization-current, and obviously of every other current, has nothing whatever to do. The idea then suggested itself to seek for the cause of the production of electricity in the experiment of Volta, not in the contact of two metals, but in previous chemical actions of the surrounding media on their surfaces. I have expressed the opinion that so-called contact-electricity is produced by the oxidation of the metal in contact by the oxygen of the air, just as in galvanic cells it is evolved by oxidation of zinc. If the supposition prove true, and it has proved true, the E.M.F. of his metal in contact in air must be measured and expressed by their heats of combustion."

He then points out how all Volta tension series are in oxidation order, and relates approvingly De la Rive's view that metals in air were attacked not only by water-vapour but by dry oxygen, and that electricity is produced by any kind of chemical action in proportion to the intensity of the chemical affinity. Then he gives his numerical theory and supporting experiments; and finally concludes:—"I believe we are entitled to say that no *Scheidungskraft* exists at the contact of two metals." The following must take the place of Volta's law of the evolution of electricity:—"The difference of electric potential between two metals in contact is measured by the algebraic sum of the heat-value of the chemical action going on at each."

In his theory and experiments, and all through the rest of the paper, he considers the difference of potential equal to *half* the difference of heat-values, so the above last statement must be a slight numerical slip.

The above extracts are among the most favourable. It would be easy to select passages from this, and from his other memoirs on the subject, of a more surprising character.

Professor Exner, to strengthen his position, adduces a large number of very simple experiments (such as connecting first one Daniell and then two Daniells to an electrometer, and observing that in the second case the deflection is double the first), and from them he obtains equations proving algebraically that $Zn / Cu = 0$. Considered as conundrums these equations are ingenious, but it is a waste of time seriously to discuss them as Herr Julius has done in an elaborate manner. To suppose that such everyday experiments as these are in direct contradiction of the contact theory is scarcely complimentary to the great men who have held, and who still hold, that view.

Dr. C. G. Knott, in 1879*, examined the contact-force between plates of the same metal at different temperatures, using the condenser or Kohlrausch method. He found that iron, copper, zinc, and probably tin, were negative when hot to the same metals cold; and the effect increases uniformly with temperature. But it is permanent, remaining after the hot plates have cooled down; hence it must be due to oxidation.

A slow oxidation proceeds with time alone. Time-curves are logarithmic like cooling-curves, and the most oxidizable metal varies most quickly both for time-variation and temperature-variation. There seems to be a surface-condition of a metal proper to each temperature which no polishing can change, for it establishes itself in a few seconds after cleaning, and only changes with temperature.

Mr. S. Lavington Hart, in 1881†, describes a mercury-dropper where the mercury is contained in a funnel, and is connected with an electrometer by an iron rod dipping into it. The drops form inside an iron inductor, and they fall negatively charged. Mr. Hart so far ignores any Volta force that he considers the arrangement as an inversion of Lippmann's electrometer, the advancing drops being oxidized. It can plainly be regarded, however, as a mere Fe/Hg contact arrangement; and that is what I suppose it to be. He makes two interesting modifications: the first is to replace the air round the dropping mercury by coal-gas; the electrical effect is then zero. This is interesting, because the exuding drops of mercury, unlike most pieces of metal, expose to the coal-gas a virgin surface which has probably contracted no condensed air-sheet: only coal-gas is a rather sophisticated substance for

* Knott, Proc. R. S. Edinb. 1879-80, No. 105, p. 362.

† Hart, Brit. Assoc. York, p. 555; and Phil. Mag. November 1881, ser. 5, xii. p. 324.

it to be first exposed to. If the experiment is regarded as sufficiently direct and simple, this fact lends support to the view that Volta forces depend on the medium surrounding the metals.

The second modification is to bring an earth-connected iron bar close to the drops, and to show that it reduces the deflection. Mr. Hart thinks it reduces the oxidation by proximity; and certainly, provided the obvious action of a mere electrostatic screen has been considered and provided against, the action by proximity is very remarkable. A similar effect has been observed and more fully worked out by Pellat, in a paper published in 1882*. Pellat says that if he places two metallic surfaces parallel to one another and very close together (say half a millimetre more or less, variations from 12 to .1), each metal undergoes a slight alteration of the properties of its superficial coat. The alteration takes some minutes to produce, increases with time, but tends to a limit. When the influencing metal is removed, the other returns gradually to its primitive state. Lead and iron produce the largest influence effects; copper, gold, and platinum give smaller but distinct effects; zinc produces hardly any, unless it be put within a hundredth of a millimetre or so. Pellat does not attempt to account for this interesting phenomenon further than by suggesting some possible connection with the smell of metals.

Mr. Hart's theoretical views are at first sight somewhat analogous to my own, though they are by no means the same. He considers the case of two metals immersed in liquid electrolytes, and dismisses air by calling it a gaseous electrolyte. He believes zinc and copper in contact to be at the same potential, and throws the variation of potential on the air between them†. He considers the electrical effect brought about by the electro-negative ion oxygen combining with the zinc and charging it negatively, while some electro-positive ion combines with the copper and charges it positively, "though not unless the two metals are in sufficient proximity to overcome electrolytic diffusion" (whatever that may mean as applied to this case). He thinks his mercury-dropping experiment in coal-gas is conclusive as to the equality of potential of metals in contact. This, I fear, is rather rapid induction.

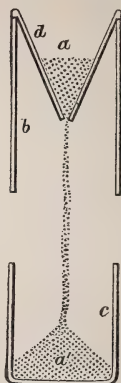
* *Comptes Rendus*, xciv. (1882) p. 1247: "Influence of Metals on one another at a Distance."

† The diagrams of potential which Mr. Hart gives of cells were given more fully by Prof. Exner in his paper on the Theory of Galvanic Elements (1880). Mr. Hart's views are, in fact, rather similar to some of the more reasonable ones of Prof. Exner.

I do not see how it follows on his own hypothesis that his arrangement is virtually a reversed Lippman electrometer.

Sir W. Thomson's dropping arrangements, or voltaic cells, in which gravity does the work instead of chemical action, are so well known that it is scarcely necessary to do more than refer to them. Mr. Hart's mercury-dropper is scarcely a modification of the copper-filing dropper shown in fig. 12. Sir William also shows how to couple up such cells in series*, and how to construct a mechanical replenisher on the Volta principle (fig. 13).

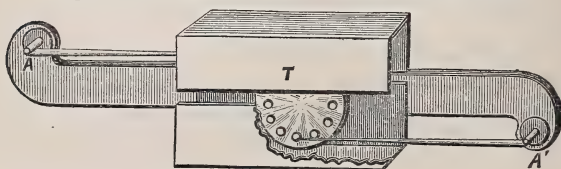
Fig. 12.—Thomson's Gravitation Voltaic Cell.



a copper filings. *c* receiver.
b inductor—zinc. *d* copper funnel.

The copper filings drop negatively charged against electrical forces. If you join *c* and *d* by a copper wire, you can get a current flowing wholly through and with copper.

Fig. 13.—Thomson's Voltaic Induction Machine.



One of the inductors, *T*, is lined with one metal, the other with another, and the two connected. The carrier-wheel is rotated, and the contact-springs, *A*, *A'*, become oppositely charged. By afterwards charging the inductors with a Daniell cell, and comparing the deflection now produced in an electrometer connected to *A*, *A'* with what it was before, measurements of Volta effect can be obtained; or of course it can be made a null method.

6. In order to give this historical sketch more completeness, it may be as well to record rapidly such other memoirs as I have been able to get acquainted with: it is in the highest degree probable that several are omitted, but I hope no very important ones. Prof. Wiedemann's collection of views and memoirs bearing on the subject is at the end of the second volume of the new edition of his *Elektricität*.

* Electrostatic Reprints, p. 325.

Edlund has published a long paper* in which he investigates experimentally the Peltier effect; he points out clearly at the end that there is no relation between the Peltier and Volta effects; and he suggests that this is because of the contact-force between the metals and the gas or air in which they are, the fact of such contact-force being, he thinks, sufficiently established by gas-batteries and galvanic polarization†.

Majocchi, in a paper printed in *Phil. Mag.* xxx. p. 97, regards the E.M.F. of contact as due to the "adhesion" of the two metals for each other: pretty much the same idea as Sir W. Thomson's chemical action at a distance, an idea which makes the energy of the Volta effect Zn/Cu depend on, and be calculable from, the combination-heat of zinc and copper in making brass. I must return to this matter later, because it is important in itself and crucial as regards theory.

Gassiot‡ made an experiment intended to show that there could be a difference of potential excited between metals by proximity without actual contact, or at any rate without metallic contact. Grove§ also made a similar experiment.

Hoorweg||, and also Nobili¶, have a theory that all galvanic currents are really thermoelectric.

In the article "Electricity" in the *Encyc. Brit.* p. 99, Professor Chrystal gives some clear general considerations regarding the seat of E.M.F. and the opposing views which are held with regard to it. He is judicial in his attitude with regard to them; but the mere statement of the position in so

* Edlund: *Pogg. Ann.* cxxxvii. p. 474; cxl. p. 435; cxliii. pp. 404, 534. See also *Phil. Mag.* [4] xxxviii. p. 263; xliii. pp. 81, 213, 264, especially p. 273.

† Sundell investigates the E.M.F. of alloys in contact with copper, employing Edlund's method, and finds, like him, that for alloys as well as for simple metals the Peltier corresponds with the Seebeck force. The peculiar language used in this and the preceding paper may easily cause it to be imagined that they have found Volta force to agree with Peltier. In fact, Sundell is so quoted in Watts's 3rd Suppl. p. 708. Von Zahn quotes Edlund in the same sense; and indeed it is probable that Edlund himself at first thought he was investigating Volta forces thermoelectrically (*Pogg. Ann.* cxlix. p. 144).

‡ Gassiot, *Phil. Mag.* xxv. 1844, p. 283.

§ Grove, 'Literary Gazette,' Jan. 21, 1843; Wiedemann, *Elek.* ii. p. 988.

|| Hoorweg, *Wied. Ann.* ix. p. 552, 1880; xi. p. 233; and xii. p. 75.

¶ Prof. Wiedemann notes as interesting that in 1828 Nobili held a notion that all galvanic currents are thermoelectric, thus vaguely anticipating the modern thermodynamic theory of E.M.F. See *Wied. Elektricität*, ii. p. 985, and Nobili, *Bibl. Univ. de Genève*, xxxvii. p. 118. But Prof. Hoorweg seems bitten with the same idea in recent times, and in 1879-80 writes long papers in proof that all current-energy is due to absorption of heat at junctions!

clear a form is in itself a powerful argument for the views held by Maxwell*.

Prof. Fleeming Jenkin, in the last edition of his 'Electricity and Magnetism,' p. 216, endeavours to reconcile the contact and chemical theories. According to the chemical theory, the E.M.F. of a cell = $\Sigma(J\theta\epsilon)$; according to the contact-theory it is $C/L + L/Z + Z/C$. On these undoubted facts he proceeds to found a number of statements which are true†, though scarcely simple; in fact they perhaps rather tend to complicate what may be held to be a simple matter.

Schönbein, in a letter to Faraday published in the *Philosophical Magazine* for 1838‡, throws out a remarkable sugges-

* Although this article is, or ought to be, easily accessible to everybody, there is one important suggestion in it which it is as well to quote, viz. that contained in the following sentence:—"We are so ignorant of the nature of the motion which is the essence of the electric current that the very form in which we have put the question [as to the locality of E.M.F.] may be misleading. If this motion be in the surrounding medium, as there is great reason to believe it to be, it would not be surprising to find that speculations as to the exact locality of the E.M.F. *in the circuit* were utterly wide of the mark." Prof. Willard Gibbs suggested something of the same sort at Montreal, though in a rather vaguer form. I do not myself feel any doubt that a precise location can be given to the E.M.F., notwithstanding that much of the current energy exists in the medium. The most complete attention to the distribution of energy in circuits which has yet been bestowed on the subject has been given by Prof. Poynting in his remarkable memoir (*Phil. Trans.* 1884), and he therein locates the E.M.F. of a battery exactly where I do myself.

† Except, indeed, a doubtful statement at the end of Number 2 and an erroneous bit of reasoning at the end of Number 4, though the conclusion drawn is correct.

‡ Schönbein, *Phil. Mag.* vol. xii. pp. 225 and 311. The two most striking sentences are here extracted:—

"Before closing my letter, allow me to communicate to you in a general manner the view which I have taken of the subject in question. In the first place, I must tell you that I am by no means inclined to consider mere contact in any case as the cause of the excitement of even the most feeble current. I maintain, on the contrary, in accordance with the principles of the chemical theory, that any current produced in a hydro-electric voltaic circle is always due to some chemical action. But as to the idea which I attach to the term 'chemical action,' I go further than you and M. de la Rive seem to go; for I maintain that any tendency of two different substances to unite chemically with one another must be considered as a chemical action, be that tendency followed up by the actual combination of those substances, or be it not; and that such a tendency is capable of putting electricity into circulation."

And on page 314 he explains this last phrase, which he has elsewhere called a "current of tendency," thus:—

"As what I term a current of tendency is no doubt in some cases nothing but that electrical state which the Voltaists consider to be the effect of their 'force electromotive,' or of contact, it appears to me that, from some of the facts above stated, a specific and most important conclusion regarding the theory of the pile can be drawn. Even if we

tion with regard to "chemical tendency" as the possible source of a current, or rather of "force electromotive." His language and ideas are in many respects old-fashioned and erroneous; he uses such phrases as "a current of tendency," he supposes currents with no electrolytic power to exist, and of course is not troubled about energy considerations. But I feel little doubt that, had he lived later, he would have held that while *currents* were due to chemical action, *electromotive force* was due to "chemical tendency;" and this is pretty exactly my own view of the matter.

I have only just discovered this Schönbein letter; and I have also found some paragraphs in Faraday which more in detail and with fair distinctness express what I believe to be the true view. (See §§ 893-900, Exp. Res. vol. i.)*

grant to the Voltaists our current of tendency to be the effect of mere contact, the facts alluded to prove that such a current does not possess a sensible degree of electrolyzing-power; consequently that the chemical effects of the common voltaic arrangements have nothing to do with current-electricity excited by contact."

* Extract from Faraday's 'Experimental Researches,' vol. i. :—

"(893) The use of *metallic contact* in a single pair of plates and the cause of its great superiority above contact made by other kinds of matter, become now very evident. When an amalgamated zinc plate is dipped into dilute sulphuric acid, the force of chemical affinity exerted between the metal and the fluid is not sufficiently powerful to cause sensible action at the surfaces of contact, and occasion the decomposition of water by the oxidation of the metal, although it is sufficient to produce such a condition of the electricity (or the power upon which chemical affinity depends), as would produce a current if there were a path open for it; and that contact would complete the conditions necessary, under the circumstances, for the decomposition of water.

"(894) Now the presence of a piece of platina touching both the zinc and the fluid to be decomposed opens the path required for the electricity. Its *direct communication* with the zinc is effectual, far beyond any communication made between it and that metal (*i. e.* between the platina and zinc) by means of decomposable conducting bodies, or, in other words, *electrolytes*, as in the experiment already described [that of decomposing iodide of potassium without metallic contact by interposing it on blotting-paper between the platinum and the zinc of a simple voltaic cell], because, when *they* are used, the chemical affinities between them and the zinc produce a contrary and opposing action to that which is influential in the dilute sulphuric acid; or if that action be but small, still the affinity of their component parts for each other has to be overcome, for they cannot conduct without suffering decomposition; and this decomposition is found *experimentally* to react back upon the forces which in the acid tend to produce the current, and in numerous cases entirely to neutralize them. Where direct contact of the zinc and platina occurs, these obstructing forces are not brought into action, and therefore the production and the circulation of the electric current, and the concomitant action of decomposition are then highly favoured.

"(895) It is evident, however, that one of these opposing actions may be dismissed and yet an electrolyte be used for the purpose of completing

Various observations regarding the E.M.F. of different cells are made in the series of papers still appearing in the *Philosophical Magazine* by Dr. Alder Wright and Mr. C. Thompson, "On the Determination of Chemical Affinity in terms of E.M.F."

For much discussion of contact-electricity and for some interesting statements of the views of Marianini, Davy, and others, refer to 'Experimental Researches,' vol. ii. p. 20 &c. From what is there said, it appears that Karsten and Marianini held a modified contact theory, placing the E.M.F. at the metal-fluid junction; and that Becquerel admitted as a possibility the efficiency of chemical attraction, as distinct from combination, something in the same way as Schönbein.

Professor Tait in his 'Thermodynamics' lends his powerful support to the contact view of the activity of the pile as taught by Sir W. Thomson.

Some work has been done in the direction of observing reversible heat-effects at metal-liquid junctions, notably by Joule, Thomson, and Bosscha. We shall have occasion to refer to this work later.

Joule in 1841 sent currents through several dilute acid voltameters with different electrodes, and measured the excess or reversible heat $H - RC^2$ generated in the whole cell; with the result that the excess of heat observed is that due to the observed back E.M.F. of the cell, minus that concerned in the decomposition of water. A table of his results is given by Chrystal, *Encyc. Brit.* p. 91. For Maxwell on the same subject see 'Elementary Electricity,' p. 146.

Thomson (Math. and Phys. Papers, pp. 496, 503) says that, of two decomposition-cells, one with zinc cathode, the other with platinum cathode, the former showed the most heat when the same current was sent through both. Separation

the circuit between the zinc and platina immersed separately into the dilute acid; for if in the above experiment the platina wire be retained in metallic contact with the zinc plate, and a division of the platina be made elsewhere, then the solution of iodide placed there, being in contact with platina at both surfaces, exerts no chemical affinities for that metal; or, if it does, they are equal on both sides. Its power, therefore, of forming a current in opposition to that dependent upon the action of the acid in the vessel is removed, and only its resistance to decomposition remains as the obstacle to be overcome by the affinities exerted in the dilute sulphuric acid.

"(896) This becomes the condition of a single pair of active plates where *metallic contact* is allowed. In such cases, only one set of opposing affinities are to be overcome by those which are dominant in the vessel; whereas, when metallic contact is not allowed, two sets of opposing affinities must be conquered (894)."

ting the electrodes by a porous cell, zinc cathode showed more heat than zinc anode, but platinum anode more heat than platinum cathode*. He speaks of the local heat developed at a tin surface, and shows that it is greater where hydrogen is liberated than where tin is dissolved; and suggests thermal observations on four dilute-acid voltameters in one circuit with zinc and platinum electrodes, arranged according to the permutations—zinc-zinc, zinc-platinum, platinum-zinc, and platinum-platinum. Thomson attributes the extra heat at an electrode to opposing chemical affinities which have to be overcome—a doctrine of “chemical resistance.”

Bosscha examines and develops all these matters in a series of interesting papers published about 1857†. He attributes the development of local heat, at a cathode against which hydrogen is liberated, to the change of hydrogen from the nascent condition to the ordinary one—in other words, to the energy of the molecular combination H, H . He finds the electromotive forces exhibited by this local generation of heat at the surface of different metals in acid to have the following values in volts :—

Pt	Fe	Cu	Sn	Hg	Zn
·45	·49	·64	·86	1·2	1·2

One more memoir I must mention before closing this historical sketch and discussion thereon, a valuable communication by Bouty to the *Journal de Physique*‡, “On Thermoelectric Force at Contact of Metals and Liquids, and on the Peltier Effect thereat.” He finds the Peltier coefficient at a junction of copper with salts of copper eighty times as great as at an iron-zinc junction, and eleven times as great as bismuth-copper. He also measures the metal-liquid thermoelectric E.M.F. at different temperatures, and shows that Thomson’s thermodynamic formula

$$J\Pi = \frac{TdE}{dT}$$

is perfectly true and in agreement with experiment in these cases also. He endeavours to see if this Peltier, or, as we had better call it for distinction, Joule or Bouty effect, can be calculated from the energies of combination. After tabulating his results alongside of heats of oxidation and heats

* Showing, I suppose, that while zinc attracts oxygen much and hydrogen not at all, platinum attracts hydrogen more than oxygen.

† Bosscha, *Pogg. Ann.* vols. ci., ciii., cv., cviii.

‡ Bouty, *Journal de Physique*, 1879, viii. p. 341; ix. pp. 229 and 306, especially p. 306.

of solution, he decides that it is hopeless, and that we must give up trying to establish a relation between these quantities. Chemical action, he concludes, only disturbs the effect by altering the surfaces, and by developing parasitic heats. They may mask, but they do not produce, the true Bouty phenomenon, which he believes is probably physical.

The difficulties of making these measurements are exceedingly great; and, notwithstanding the ingenuity and skill displayed, it seems to me possible that some error or unexpected source of disturbance may have modified the results. So far as I know, they have not yet been repeated; and I can hardly regard the experimental method used as perfectly safe*.

[To be continued.]

XXI. *Some Principles and Results of Harmonic Motion.*

By PLINY EARLE CHASE, LL.D.†

WHEN two bodies, in relative motion, come into contact, pressure begins to act between them to prevent any parts of them from jointly occupying the same space. . . . Any force in a constant direction acting in any circumstances, for any time great or small, may be reckoned on the same principle; so that what we may call its whole amount during any time, or its '*time-integral*,' will measure, or be measured by, the whole momentum which it generates in the time in question" (Thomson and Tait, '*Natural Philosophy*,' i. sections 294, 297).

No relative motions can be more important, and no contact more complete, than those which exist between the all-pervading luminiferous æther and the sun. In the conversion of tangential luminous waves into spherical vibrations, gravity is acting in a constant direction towards the sun's centre. The amount of its activity upon each æthereal particle, μ , or its time-integral during each cyclical period, t , is measured

* I find that a method exactly like that used by Bouty was suggested by Clerk Maxwell, '*Elementary Electricity*,' p. 146.

† From the *Journal of the Franklin Institute*, January 1885. Communicated by the Author.

This paper has been written, in part, to meet a desire which has been expressed by some members of the Royal Astronomical Society for an introduction to the article on "*Harmonic Motion in Stellar Systems*" (*Phil. Mag.* Sept. 1884). The author will feel under obligation to any readers who will inform him of other points which need further elucidation.

by $\mu g t$. The study of the various correlations which flow from this integral involves the following considerations:—

(1) The principle of Galileo, that the total effort is equivalent to the effective sum of the causes which are operating. This is illustrated at sun's surface, which is the region of greatest energy in our system, by the cyclic equation

$$\Sigma g = g t = v_{\lambda}.$$

(2) The equality of pressures or resistances to opposing forces, in all equilibrating tendencies. In consequence of this equality, every interruption or prevention of the free action of any force may be measured as a rate of change of momentum in the opposite sense.

(3) The invariability of the sum of kinetic and potential energies.

(4) The maximum of kinetic energy in all cyclical motions, at the point of the trajectory which is nearest to the centre of force, and the maximum of potential energy at the point which is most remote from that centre. At the origin of luminous radiation, where $\Sigma g = v_{\lambda}$, the reaction of gravitating resistance is all kinetic; after a half-rotation it becomes all potential, but it is then combined with a new reaction which is all kinetic.

(5) The evidences of local æthereal stress, which is operative in various forms of gravitating, thermal, electromagnetic, and chemical attraction and repulsion.

(6) The indestructibility of energy, which requires, as Maxwell has stated, that energy must exist in the æther during the interval of its transfer from one body to another.

(7) The strong reasons for believing that the media for luminous, gravitating, thermal, electromagnetic, and chemical interactions, occupying the same space and represented by the same cyclical velocities, are identical.

(8) The counteraction of stress by obvious elasticity, as in a spring, or by probable intermolecular elasticity and orbital motion, as in the resistance of solids.

(9) The principle of Fourier, that any complex periodic motion must be compounded of a definite number of simple harmonic motions, of definite periods, definite amplitudes, and definite phases.

(10) The application of "Laplace's Coefficients," or spherical harmonic analysis, to the explanation of cyclical waves and vibrations in spherical elastic masses.

(11) Cumulative and progressive undulating tendencies, resulting in complex harmonic motions of various kinds.

The evidences and consequences of harmonic motion in

stellar systems flow simply and naturally from the two following important FACTS:—

(1) The fundamental velocity in the solar system is the velocity of light.

(2) This is not a merely accidental and temporary coincidence, but, according to the first proposition of Newton's *Principia*, it must have existed, and must continue to exist, during all past and future stages of solar condensation.

The first of these facts was discovered by investigating the tendencies to oscillatory nodal harmony in elastic media. It appears to be still regarded by some persons as merely curious and without important significance. This is, perhaps, because attention has been so often directed to the angular velocities in different orbits, which vary inversely as the $\frac{3}{2}$ power of the mean distance, that the synchronous variability of angular velocity with the inverse square of the radius, in all the particles of an expanding or contracting nebular nucleus, is forgotten or overlooked.

We find in our stellar system three stages of material aggregation:—(1) A nucleus, probably consisting chiefly, if not entirely, of condensed gas, rotating in about 25·5 days. (2) An atmosphere of lighter gas, rotating synchronously with the nucleus, and therefore extending no further than Kant's limit ($\rho_k = 36\cdot35\rho_0$, ρ_0 being Sun's semidiameter). (3) A luminiferous æther which, if infinitely elastic, may be of infinite extent, but which would reach, if made homogeneous, to 2210·74 times Earth's mean distance from the Sun.

If the Sun were homogeneous and unresisting, a particle would fall to its centre from its surface, or from any point within its surface, in 41·87 minutes, or in $\frac{1}{4}$ of the time of

free revolution at the surface ($\frac{1}{4}$ of $2\pi\sqrt{\frac{r}{g}} = \frac{\pi}{2}\sqrt{\frac{r}{g}}$). The

relation of radial oscillation to orbital revolution is therefore that of simple harmonic motion. The velocity acquired by falling from the surface to the centre would be \sqrt{gr} ; which is the same as would be acquired in falling from an infinite distance to $2r$, or from $2r$ to r , or in virtual fall through $\frac{1}{2}r$, or through one half the length of a cycloidal pendulum which would oscillate synchronously with circular orbital revolution at r . It is also the velocity of wave-propagation in a homogeneous elastic atmosphere of a depth equivalent to r .

At all points within the nucleus the velocity acquired by fall to the centre would be proportional to the distance from the centre. Let us apply the principles which are here indicated to a stellar system like our own, with a nucleus so

preponderating that its oscillatory tendencies are not materially modified by the perturbations of attendant planets or companion stars.

Within our solar nucleus all the tendencies to orbital revolution have been converted into constrained rotation, which shows a retardation by collision, subsidence-oscillations, &c., such that the velocity of every particle is only about $\frac{1}{219}$ as great as that of free and self-sustained revolution. The continual violent agitation of Sun's mass indicates the probability of synchronous radial and tangential oscillations, which are dependent upon simple harmonic motions. The measure of gravitating acceleration for the oscillatory unit of time being $\pi^2 l = \frac{3\pi^2 l_0}{2}$, (l_0 being the distance from the point of suspension to the centre of oscillation in a linear pendulum l), we have Sun's semidiameter $(\rho_0) = \frac{3\pi^2}{2} l_0$, and the quotient of circular orbital velocity by equatorial velocity of rotation

$$(\sqrt{g_0 \rho_0} \div v_r) = \left(\frac{3\pi^2}{2} \right)^2 = 219 \cdot 17.$$

The principle of conservation of areas requires that the angular velocity should vary inversely as the square of radius, without the nucleus as well as within it, provided we look merely at the orbit of any given mass or particle. It is only when we compare different extra-nuclear orbits in the same system, each of which has its own original force of projection, that we find angular velocities varying inversely as the $\frac{3}{2}$ power of the mean distance. If the nucleus is expanding or contracting, the Kantian radius, ρ_κ , should, accordingly, vary as the $\frac{4}{3}$ power of the nucleal radius, so that

$$\rho_\kappa = \left(\frac{3\pi^2}{2} \right)^{\frac{4}{3}} \rho_0 = 36 \cdot 35 \rho_0.$$

The free undulatory velocity at ρ_0 , both radially and tangentially, is $\sqrt{g_0 \rho_0}$. The mean harmonic radial velocity of rotary oscillation at the same point is $\frac{1}{\pi}$ of $\left(\frac{3}{2\pi^2} \right)^2 = \frac{1}{688 \cdot 54}$ as great, or $\left(\frac{1}{688 \cdot 54} \right)^2$ as great as the fundamental velocity, or the gravitating acceleration in the fundamental oscillatory unit of time.

As the idea of "fundamental velocity" will be new to many readers it may be well to explain it. In whatever way g may

act, whether by constant, successive, or undulatory pressures, thrusts or pulls, all of its results may be accounted for on the hypothesis of successive instantaneous impulses. The entire accelerating force gt is always exerted, but a portion is usually counteracted by opposing resistances or momenta. Suppose, for example, three bodies in a vacuum—A falling freely for a second, B suspended from a string between cycloidal arcs so that its centre of oscillation is $\frac{1}{\pi^2}$ of 32.08 feet from the point of suspension, C sliding on a frictionless plane in which $h:l::1:\pi^2$. A will fall 16.04 feet, while B swings once and C slides 16.04 feet, falling through a height equivalent to one half the length of the pendulum.

The choice of a second as the unit of time is altogether arbitrary. The desirability of a natural unit has often been felt and expressed. Maxwell suggested, as a suitable unit, the interval of some particular luminous wave-length. At the surface of a stellar nucleus, where gravity is a maximum, where the time of rotary oscillation can be linked with all possible times of revolution by simple formulas of harmonic vibration, and where a universal gravitating unit can be readily framed from the units of mass and density, the velocity gt , which is indicated both by the gravitating and by the æthereal unit*, may properly be regarded as a fundamental velocity.

It is at present impossible to test the oscillatory velocities in remote stellar systems; but as the considerations on which they are based are universal, there are good grounds for believing that their influence is equally universal, so that $gt=v_\lambda$ at every stellar surface. In the solar system we find

$t_\kappa = 1101227 \text{ sec.} = \text{oscillatory unit of time} = \text{one half solar rotation.}$

$g_\kappa = \pi^2 \rho_\kappa = \pi^2 \left(\frac{3\pi^2}{2} \right)^{\frac{4}{3}} \rho_0 = \text{unit of gravitating acceleration in unit of time at } \rho_\kappa.$

$g_0 = \pi^2 \left(\frac{3\pi^2}{2} \right)^{\frac{4}{3}} \rho_0 = \text{unit of gravitating acceleration at } \rho_0 \text{ in oscillatory unit of time} = v_\lambda \text{ in oscillatory unit of time} = \text{solar modulus of light } (=185505 \text{ miles per sec.}) = 185505 \times 1101227 \text{ miles} = (688.54)^2 \rho_0.$

* The time of virtual fall through one half homogeneous æthereal atmosphere.

$g_0 \div \pi \left(\frac{3\pi^2}{2} \right)^2 = \sqrt{g_0 \rho_0} = v_0 =$ radial velocity acquired by falling from ρ_0 to the centre of a homogeneous nebular sphere = self-sustained velocity at surface of the sphere (= 269.42 miles per sec. at Sun's surface) = 269.42×11011227 miles in t_κ .

$\sqrt{g_0 \rho_0} \div \frac{\pi}{2} \left(\frac{3\pi^2}{2} \right)^2 = v_p =$ simple harmonic radial component of stellar equatorial velocity of rotation (= .7826 mile per sec. for our Sun) = $2\rho_0 \div \pi$ in t_κ .

The cumulative influence of mean luminous undulation and solar action is shown by the proportions

$$v_p : v_0 :: v_0 : \frac{1}{2} v_\lambda,$$

$$v_p : v_\pi :: v_\pi : v_\lambda.$$

In the second of these proportions v_π is parabolic velocity, or velocity acquired by infinite fall, at Sun's surface.

In studying planetary harmonies, regard should be paid to the following cyclical tendencies:—

$t_1 \propto (d)^{\frac{1}{2}}$ in simple oscillation under a constant force.

$t_2 \propto d$ in cyclical motions which are immediately determined by luminous undulation, or by other constant velocities.

$t_3 \propto d^{\frac{3}{2}}$ in different orbits, under different original forces of projection but with the same central force.

$t_4 \propto d^2$ in a rotating and uniformly expanding or contracting nucleus.

If d in each instance is equivalent to the Kantian limit (ρ_κ), and if the mean density of the rotating mass is represented by δ ,

$$t_1 \propto \left(\frac{1}{\delta} \right)^{\frac{1}{2}}, \quad t_3 \propto \left(\frac{1}{\delta} \right)^{\frac{3}{2}},$$

$$t_2 \propto \left(\frac{1}{\delta} \right)^{\frac{1}{3}}, \quad t_4 \propto \left(\frac{1}{\delta} \right)^{\frac{2}{3}}.$$

If the mean density of Earth's Kantian rotation is taken as the unit, the corresponding reciprocals of density for Sun and for the several planets, except Neptune, are nearly as follows:—

Sun	649.800		Mars	1.053
Mercury	1.207		Jupiter171
Venus	1.031		Saturn182
Earth	1.000		Uranus157

The influence of luminous undulation upon the actions and reactions between the chief centre of nucleation and the chief centre of condensation (Sun and Earth) seems to be indicated by the following tendencies:—

t_2 , in the conversion of circular motion into simple harmonic motion, $\propto \frac{2}{\pi}d$. d_3 representing such conversion, $\propto \left(\frac{2}{\pi}\right)d$.

The radius of mean condensation ($\frac{1}{2}\delta$) is $2^{\frac{1}{2}}$ times the radius of actual condensation; $365\cdot256 \times \left(\frac{2}{\pi}\right)^{\frac{2}{3}} \div 2^{\frac{1}{2}} = 214\cdot54$. The apparent solar semidiameter which corresponds to this value is $961''\cdot43$, which differs by less than $\frac{1}{23}$ of 1 per cent. from the British Nautical Almanac estimate, $961''\cdot82$. Dr. Fuhg's estimate (*Astron. Nachr.* 2040), $961''\cdot495$, is still closer.

Tendencies to solar equatorial acceleration and polar retardation, such as are shown by the sun-spots, are indicated by the following approximations to the period of solar rotation:—

$$t_1 \propto \sqrt{d}; (36\cdot3514 \times 430891) \div (6\cdot6 \times 3962\cdot8) = (24\cdot472)^2.$$

$$_2 \propto d; 430891 \div (3962\cdot8 \times 3\cdot92295) = 27\cdot7174.$$

$$t_3 \propto \sqrt{\delta}; (36\cdot3514 \div 6\cdot6)^3 \times 3\cdot92295 = (25\cdot602)^2.$$

$$t_4 \propto d^2; (36\cdot3514 \div 6\cdot6)^3 = 30\cdot336.$$

The secular influence of the chief centres of nucleation and nebulosity (Sun and Jupiter) on the chief centre of condensation (Earth), as well as the cyclical actions and reactions between Earth and Jupiter, are shown by the equation

$$\delta_5 \rho_{3\beta} \div \delta_0 \rho_{3a} = t_\beta \div t_a.$$

In this equation δ_5, δ_0 are the mean Kantian densities of Jupiter, Sun; $\rho_{3\beta}, \rho_{3a}$, the nearest secular approaches of Earth to Jupiter, Sun; t_β , orbital period of Jupiter; t_a , Kantian or rotational period of Earth. The secular eccentricity of Earth which satisfies this equation is $\cdot06543$. Stockwell's estimate is $\cdot06774$.

XXII. *On the Determination of Chemical Affinity in terms of Electromotive Force.*—Part IX. By C. R. ALDER WRIGHT, D.Sc. (Lond.), F.R.S., Lecturer on Chemistry and Physics, and C. THOMPSON, F.C.S., Demonstrator of Chemistry, in St. Mary's Hospital Medical School*.

[Concluded from p. 124.]

D. *Voltaic and Thermovoltaic Constants of Metals immersed in Solutions of their Acetates.*

I. Copper.

194. A NUMBER of cells were set up with amalgamated-zinc and electro-copper plates immersed in solutions of their respective acetates, the constant molecular strength being $\cdot 25 \text{ M}(\text{C}_2\text{H}_3\text{O}_2)_2 \text{ H}_2\text{O}^*$. The following values were obtained :—

Maximum	1·104
Minimum	1·073
Average	1·091
Probable error	·0029

Julius Thomsen's thermochemical data lead to the values $\text{Zn, O, } 2\text{C}_2\text{H}_4\text{O}_2\text{aq.} = 100710$, and $\text{Cu, O, } 2\text{C}_2\text{H}_4\text{O}_2\text{aq.} = 50340$; whence $E_{\text{H}} = 1\cdot111$, corresponding with 50370 gramme-degrees. Hence $E - E_{\text{H}} = -\cdot 020$ for electro-copper in contact with copper-acetate solution of strength $\cdot 25 \text{ Cu}(\text{C}_2\text{H}_3\text{O}_2)_2 100 \text{ H}_2\text{O}$.

II. Lead.

195. Two series of cells set up with electro-lead opposed to amalgamated zinc and electro copper respectively, with acetate solutions uniformly of strength $\cdot 25 \text{ M}(\text{C}_2\text{H}_3\text{O}_2)_2 100 \text{ H}_2\text{O}$, gave the following results ; the differences observed between the values obtained for different cells set up in the same way being notably greater than with most of the other combinations examined :—

	Zinc-Lead.	Lead-Copper.
Maximum	·616	·503
Minimum	·587	·471
Average	·608	·485
Probable error	$\pm \cdot 0048$	$\pm \cdot 0044$

* The acetate solutions employed were prepared by dissolving the freshly-precipitated, well-washed carbonates of the metals in boiling dilute acetic acid somewhat stronger than that required, filtering after cooling, and diluting to the proper extent.

From these average values the following valuations result for the voltaic constant for lead in acetate solution of strength $\cdot 25 \text{ Pb}(\text{C}_2\text{H}_3\text{O}_2)_2$ 100 H_2O :—

Zinc-lead $\cdot 608$

Zinc-copper—Copper-lead . . $\left\{ \begin{array}{l} 1\cdot 091 \\ -\cdot 485 \end{array} \right\} = \cdot 606$

Mean $\cdot 607$

Hence, since Thomsen finds that $\text{Pb}, \text{O}, 2\text{C}_2\text{H}_4\text{O}_2 \text{ aq.} = 65770$, the value of E_{H} is $\cdot 770$, corresponding with 34940 gramme-degrees, whence

$$E - E_{\text{H}} = -\cdot 163;$$

i. e. the thermovoltic constant for lead in acetate solution is negative, and of nearly the same magnitude as in nitrate solution, or in contact with lead sulphate suspended in zinc or cadmium-sulphate solution of corresponding strength, $\cdot 25 \text{ PbX}$ 100 H_2O . In consequence the actual E.M.F. of a lead-copper-acetate cell *exceeds* that due to the net chemical change taking place therein, just as in lead-copper-sulphate and lead-copper-nitrate cells.

Some observations were made on the rate of depreciation in E.M.F. with increasing current-density in zinc-lead-acetate cells, with the result of showing that the average rate of fall is indicated by a curve lying between the analogous curves obtained with zinc-silver-sulphate and iron-copper-sulphate cells (§§ 130 and 176), being below the former and above the latter, which, as already shown, overlie the analogous curves obtained with zinc-copper-sulphate and zinc-cadmium-sulphate cells. It is remarkable, in this connection, that the thermovoltic constants for the metals silver, lead, iron, copper, and cadmium in these solutions respectively follow in the same order as regards numerical magnitude, though not as regards sign:—

Silver (in sulphate solution) . . $-\cdot 351$

Lead (in acetate solution) . . $-\cdot 163$

Iron (in sulphate solution) . . $+\cdot 113$ to $+\cdot 148$

Copper (in sulphate solution) . . $+\cdot 009$

Cadmium (in sulphate solution) $+\cdot 005$ to $-\cdot 010$

III. Silver.

196. The following values were obtained with cells containing electro-silver immersed in saturated silver-acetate solution opposed to amalgamated zinc and electro-copper immersed in their respective acetate solutions, the constant strength being $\cdot 057 \text{ M}(\text{C}_2\text{H}_3\text{O}_2)_2$ 100 H_2O :—

trace out whether the different halogen salts exhibit analogy with these oxygen-acid salts, or whether they differ materially amongst themselves, a number of cells were examined containing various metals immersed in solutions of their respective bromides of constant strength $\cdot 25 \text{MBr}_2$ 100 H_2O , or in magmas of their bromides immersed in zinc-bromide solution of this strength (saving in the case of lead bromide, where the sparing solubility necessitated the use of weaker solutions).

I. Cadmium.

The following values were obtained with zinc-cadmium-bromide cells :—

Maximum	$\cdot 319$
Minimum	$\cdot 311$
Average	$\cdot 315$
Probable error	$\pm \cdot 0014$

Julius Thomsen finds the values $\text{Zn}, \text{Br}_2 \text{ aq.} = 90960$, and $\text{Cd}, \text{Br}_2 \text{ aq.} = 75640$; whence $E_{\text{H}} = \cdot 338$, corresponding with 15320 gramme-degrees. Hence

$$E - E_{\text{H}} = -\cdot 023 ;$$

that is, the thermovoltaic constant for electro-cadmium in bromide solution of strength $\cdot 25 \text{CdBr}_2$ 100 H_2O is a small negative quantity, not quite so large numerically as that for the corresponding chloride solution (viz. $-\cdot 036$).

II. Silver.

198. The following values were obtained with cells set up with amalgamated zinc opposed to electro-silver immersed in a magma of freshly precipitated well-washed silver bromide suspended in zinc-bromide solution, the solution-strength throughout being $\cdot 25 \text{ZnBr}_2$ 100 H_2O .

Maximum	$\cdot 909$
Minimum	$\cdot 901$
Average	$\cdot 906$
Probable error	$\pm \cdot 0012$

Julius Thomsen finds $\text{Ag}_2, \text{Br}_2 = 45400$; whence $E_{\text{H}} = 1\cdot 005$, corresponding with 45560 gramme-degrees. Hence

$$E - E_{\text{H}} = -\cdot 099 ;$$

that is, the thermovoltaic constant for silver in contact with silver bromide suspended in zinc-bromide solution is a notable negative quantity, slightly less numerically than the corresponding value for silver chloride suspended in equally strong zinc-chloride solution (viz. -112).

III. Lead.

199. Two sets of cells were examined containing electro-lead immersed in saturated solution of lead bromide of strength $\cdot 037 \text{ PbBr}_2$ 100 H_2O opposed to amalgamated zinc and electro-cadmium immersed in their respective bromide solutions of the same strength. The following values were obtained :—

	Zinc-Lead.	Cadmium-Lead.
Maximum	$\cdot 575$	$\cdot 257$
Minimum	$\cdot 567$	$\cdot 255$
Average	$\cdot 571$	$\cdot 256$
Probable error	$\pm \cdot 0014$	$\pm \cdot 0001$

These figures lead to the following valuations for the voltaic constant of lead in saturated lead-bromide solution :—

Zinc-lead $\cdot 571$

Zinc-cadmium* + Cadmium-lead . . $\left\{ \begin{array}{l} 315 \\ + 256 \end{array} \right\} = \cdot 571$

Mean $\cdot 571$

Julius Thomsen finds $\text{Pb}, \text{Br}_2 \text{ aq.} = 54410$; whence $E_{\text{H}} = \cdot 806$, corresponding with 36550 gramme-degrees. Hence

$$E - E_{\text{H}} = -\cdot 235 ;$$

i. e. the thermovoltic constant for electro-lead in saturated lead-bromide solution is a large negative quantity, slightly greater numerically than that for lead in saturated lead-chloride solution, *viz.* $-\cdot 222$.

IV. Mercury.

200. When mercuric-bromide solution is agitated with metallic mercury, mercurous bromide is rapidly formed, so that in a very short time practically all mercury and bromine are removed from solution, just as is the case with mercuric chloride (§ 183). Cells were therefore set up containing mercury in contact with a magma of mercurous bromide (freshly precipitated and well washed), suspended in zinc-bromide solution, and opposed, firstly, to amalgamated zinc immersed in zinc-bromide solution, and, secondly, to electro-

* Zinc-cadmium assumed to give the same value for this solution-strength as that found above for the somewhat stronger solution $\cdot 25 \text{ CdBr}_2$ 100 H_2O .

silver in contact with a magma of silver bromide suspended in zinc-bromide solution, the solution-strength being $\cdot 25 \text{ZnBr}_2$ 100 H_2O throughout. The following values were obtained, the current passing, in the latter case, in the direction opposite to that predicable from the relative heats of formation of mercurous and silver bromides :—

	Zinc-Mercury.	Mercury-Silver.
Maximum	$\cdot 974$	$-\cdot 062$
Minimum	$\cdot 969$	$-\cdot 073$
Average	$\cdot 972$	$-\cdot 066$
Probable error	$\pm \cdot 0006$	$\pm \cdot 0019$

These figures lead to the following valuations for the voltaic constant of mercury in contact with mercurous bromide suspended in $\cdot 25 \text{ZnBr}_2$ 100 H_2O :—

Zinc-mercury $\cdot 972$

Zinc-silver—Mercury-silver . . $\left\{ \begin{array}{l} \cdot 906 \\ + \cdot 066 \end{array} \right\} = \cdot 972$

Mean $\cdot 972$

Julius Thomsen finds $\text{Hg}_2, \text{Br}_2 = 68290$; whence $E_{\text{H}} = \cdot 500$ volt, corresponding with 22670 gramme-degrees. Hence

$$E - E_{\text{H}} = + \cdot 472 ;$$

that is, the thermovoltaic constant for mercury in contact with this bromide magma is a large positive quantity, somewhat greater than that observed with the corresponding chloride magma, viz. $+ \cdot 458$.

It is noticeable that the heat of formation of mercurous bromide exceeds that of lead-bromide solution ($\text{Hg}_2, \text{Br}_2 = 68290$, $\text{Pb}, \text{Br}_2 \text{ aq.} = 54410$, difference 13880, corresponding with $\cdot 306$ volt); whilst the value of $k_1 - k_2$ for lead-mercury-bromide cells is

$$-\cdot 235 - (+ \cdot 472) = - \cdot 707 ;$$

so that $E_{\text{H}} + k_1 - k_2$ is negative relatively to E_{H} , being $= - \cdot 401$. On examining various such cells (solution-strength $= \cdot 037 \text{MBr}_2$ 100 H_2O), the E.M.F. was found to be actually of negative sign, the average observed value being $- \cdot 402$.

V. Iron.

201. A series of cells was examined containing plates of nearly pure sheet iron (bright) opposed to amalgamated zinc

and electro-cadmium with bromide solutions of the respective metals of strength $\cdot 25 \text{ MBr}_2$ 100 H_2O (the ferrous-bromide solutions being prepared by agitating together pure spongy iron in excess and weighed quantities of bromine and water in a stoppered bottle, filtering and using immediately). The following numbers were obtained :—

	Zinc-Iron.	Iron-Cadmium.
Maximum	$\cdot 441$	$-\cdot 086$
Minimum	$\cdot 401$	$-\cdot 126$
Average	$\cdot 417$	$-\cdot 103$
Probable error	$\pm \cdot 005$	$\pm \cdot 005$

As with the cadmium-iron sulphate and chloride cells, the current passed in the second case in the direction opposite to that predicable from the relative heats of formations of iron- and cadmium-bromide solutions ; while the E.M.F. actually set up in the first case notably *exceeded* that similarly calculable from the heats of formation of zinc- and iron-bromide solutions.

These numbers lead to the following valuations for the voltaic constant for bright iron in solution of strength $\cdot 25 \text{ FeBr}_2$ 100 H_2O :—

Zinc-iron $\cdot 417$

Zinc-cadmium — Cadmium-iron $\left\{ \begin{array}{l} \cdot 315 \\ + \cdot 103 \end{array} \right\} = \cdot 418$

Mean . . . $\cdot 418$

Julius Thomsen finds $\text{Fe}, \text{Br}_2 \text{ aq.} = 78070$; whence $E_{\text{H}} = \cdot 284$ volt, corresponding with 12890 gramme-degrees. Hence

$$E - E_{\text{H}} = + \cdot 134 ;$$

i. e. the thermovoltaic constant is a notable positive quantity, less, however, in magnitude than the corresponding value for ferrous-chloride solution of the same strength, viz. $+ \cdot 204$.

VI. Aluminium.

202. A number of cells were set up with bright aluminium plates opposed to amalgamated zinc and electro-cadmium, immersed in their respective bromide solutions of strength $\cdot 25 \text{ MBr}_2$ 100 H_2O . The readings exhibited the same fluctuations as were observed with the sulphate and chloride cells previously examined (§ 178 and § 186). The following readings were obtained, the current passing in the opposite direction to that calculable from the relative heats of forma-

204 Messrs. Wright and Thompson on the Determination of
tion of zinc- and aluminium-bromide solutions in the first
case, but in the normal direction in the second case :—

	Zinc-Aluminium.	Cadmium-Aluminium.
Maximum	—·323	+·037
Minimum	—·278	+·013
Average	—·296	+·022
Probable error	±·007	±·005

These numbers lead to the following valuations for the
voltaic constant for bright aluminium in bromide solution
·25 $\text{Al}_{\frac{2}{3}}\text{Br}_2$ 100 H_2O :—

$$\begin{array}{rcl}
 \text{Zinc-aluminium} & . & +\cdot296 \\
 \text{Zinc-cadmium} - \text{Aluminium-cadmium} & \left\{ \begin{array}{l} \cdot315 \\ -\cdot022 \end{array} \right\} & = +\cdot293 \\
 \text{Mean} & . & +\cdot295
 \end{array}$$

the constant being of + sign, since aluminium actually
acquires the higher potential, although it might *à priori* be
expected to acquire the lower potential.

Julius Thomsen's thermochemical data lead to the value
 $\text{Al}_{\frac{2}{3}}, \text{Br}_2 \text{ aq.} = 136680$; whence $E_{\text{H}} = -1\cdot008$ volt, corre-
sponding with 45720 gramme-degrees. Consequently

$$E - E_{\text{H}} = +\cdot295 - (-1\cdot008) = +1\cdot303;$$

i. e. the thermovoltaic constant for bright aluminium in
bromide solution ·25 $\text{Al}_{\frac{2}{3}}\text{Br}_2$ 100 H_2O is a large positive
quantity, slightly greater than that found with the corre-
sponding chloride solution, viz. +1·288.

F. Voltaic and Thermovoltaic Constants of Metals immersed in Solutions of their Iodides.

I. Cadmium.

203. The following values were obtained with a number of
cells set up with amalgamated zinc and electro-cadmium
immersed in solutions of their iodides of strength ·25 MI_2
100 H_2O :—

Maximum	·326
Minimum	·320
Average	·322
Probable error	±·0008

Julius Thomsen finds the heat-values for this strength of solution are $\text{Zn, I}_2, \text{aq.} = 60540$, and $\text{Cd, I}_2, \text{aq.} = 47870$; whence $E_H = \cdot 279$ volt, corresponding with 12670 gramme-degrees. Hence $E - E_H = + \cdot 043$; *i. e.* the thermovoltaic constant for cadmium in iodide solution of strength $\cdot 25 \text{ CdI}_2, 100 \text{ H}_2\text{O}$ is positive in sign but not large numerically. It is noteworthy that, with the corresponding bromide and chloride solutions, the values were negative, *viz.* $- \cdot 023$ and $- \cdot 036$ respectively; the bromide value being thus intermediate between the chloride and iodide values.

II. Silver.

204. The following numbers were obtained with cells set up with electro-silver immersed in a magma of freshly precipitated well-washed silver iodide suspended in zinc iodide solution, and opposed to amalgamated zinc or electro-cadmium in their respective iodide solutions, the solution-strength being uniformly $\cdot 25 \text{ MI}_2, 100 \text{ H}_2\text{O}$.

	Zinc-Silver.	Cadmium-Silver.
Maximum	$\cdot 713$	$\cdot 393$
Minimum	$\cdot 696$	$\cdot 374$
Average	$\cdot 7055$	$\cdot 384$
Probable error	$\pm \cdot 0028$	$\pm \cdot 0030$

These values lead to the following valuations of the voltaic constant for electro-silver in contact with this magma:—

$$\begin{array}{rcl}
 \text{Zinc-silver} & . & \cdot 7055 \\
 \text{Zinc-cadmium} + \text{Cadmium-silver} & \left\{ \begin{array}{l} \cdot 322 \\ + \cdot 384 \end{array} \right\} & = \cdot 706 \\
 \text{Mean} & . & \cdot 706
 \end{array}$$

Since Thomsen finds $\text{Ag}_2, \text{I}_2 = 27600$, the value of E_H is $\cdot 726$ volt, corresponding with 32940 gramme-degrees: hence $E - E_H = - \cdot 020$.

On comparing this value with those found for silver bromide suspended in zinc-bromide solution and for silver chloride suspended in zinc-chloride solution, all of the same strength $\cdot 25 \text{ ZnX}_2, 100 \text{ H}_2\text{O}$, *viz.* $- \cdot 099$ and $- \cdot 112$ volt respectively, it is noticeable that the direction of variation is the same as that observed in the analogous cases for cadmium, the bromide value being intermediate between the chloride and iodide values, the last being algebraically the greatest.

208 Messrs. Wright and Thompson on the Determination of value is algebraically the greatest, as was found with cadmium-silver and mercury.

It is hence evident that whilst the thermovoltaic constants of metals in contact with their oxy salts (sulphates, nitrates, and acetates) do not vary widely, the same remark is not true for their halogen salts (chlorides, bromides, iodides). In general the thermovoltaic constant is decreased (algebraically) by substitution of bromide for iodide or of chloride for bromide; although this rule is not without exception, *e. g.* in the case of iron, where the values for $\cdot 25 \text{ FeX}_2, 100 \text{ H}_2\text{O}$ are $+\cdot 204$ and $+\cdot 134$, where X represents chlorine and bromine respectively, the substitution of chlorine for bromine thus causing a notable *increase*.

Summary of Results.

207. The experiments above described lead to the general conclusion that electromotors, consisting of voltaic cells in which two different metals are surrounded by solutions of corresponding salts respectively, are capable of generating electromotive forces which (when not depreciated sensibly below their maximum value by giving rise to currents of densities greater than certain small limiting values) usually *stand in no simple relationship to the chemical action taking place in the cell during the passage of the current, or to the heat-evolution taking place during that passage, or to the heats of formation of the two solutions electrolyzed.* The values of the maximum E.M.F.'s thus generated may, however, be deduced within very close limits of accuracy by taking the algebraic difference between certain numerical values or *voltaic constants* assigned to each metal in contact with each given solution, these values varying slightly according to the surface-nature of the immersed metal, and being also dependent on the nature and strength of the solution of metallic salt in contact with the metal, and probably also varying with the temperature, but being otherwise actually constant.

Further, these maximum E.M.F. values may be connected with the difference in heat of formation between the two solutions electrolyzed by supposing that *the total difference of potential set up is due to two superposed causes*:—one, the heat-evolution due to the difference in heat of formation of the two solutions, which tends to make the metal immersed in the electrolyte of lesser formation-heat acquire the higher potential (like the copper plate of a Daniell cell); the other, a *thermo-voltaic* action akin to the *modus operandi* of an ordinary

thermovoltaic couple, in virtue of which a difference of potential is set up expressible by the algebraic difference between two numerical values, or *thermovoltaic constants*, assignable to each metal respectively, the which values vary *pari passu* with the variations in the corresponding voltaic constants. This thermovoltaic action being expressible by $k_1 - k_2$, and the first source of potential-difference being indicated by E_H , the total difference of potential set up is

$$E = E_H + k_1 - k_2,$$

where, if $k_1 - k_2$ be materially different from zero (which is most frequently the case), the value of E will differ materially from that of E_H , exceeding or falling short of it according as $k_1 - k_2$ is positive or negative in sign with respect to E_H . In the latter case, if $k_1 - k_2$ be numerically greater than E_H , the direction of the current actually generated is the reverse of that predicable from the relative heats of formation of the electrolytes.

208. The following Table furnishes a brief epitome of the mean values of the voltaic and thermovoltaic constants deduced above for various metals and fluids at a temperature within a very few degrees above or below 18°C. ; the solutions surrounding both plates being of equal strength (m zinc-salt molecules per 100 water molecules), the plate-surfaces being freshly amalgamated ones for zinc (standard), bright fused metal for iron, magnesium, and aluminium, and freshly electro-coated plates in all other cases.

Metal.	Surrounding Fluid.	m .	Voltaic constant.	E_H .	Thermovoltaic constant.
COPPER	Sulphate	·1 to 2·25	{ 1·114 invariable.	1·105 invariable.	+·009
	Nitrate	·25 to 8·0	1·066 to 1·109	1·105 to 1·100	—·039 to +·005
	Acetate	·25	1·091	1·111	—·020
	Chloride: Cu_2Cl_2 suspended in CuCl_2 solution; or CuCl_2 solution alone	·25	1·099	} 1·038 {	+·061
	Chloride: Cu_2Cl_2 suspended in CdCl_2 solution.	·25	1·001		—·037
	Chloride: Cu_2Cl_2 suspended in ZnCl_2 solution.	·25	·988		—·050
CADMIUM ...	Sulphate	·1 to 5·0	·362 to ·357	·357 to ·367	+·005 to —·010
	Nitrate	·25	·352	·357	—·005
	Chloride.....	·25 to 8·0	·330 to ·262	·366 to ·240	—·036 to +·045
	Bromide.....	·25	·315	·338	—·023
	Iodide	·25	·322	·279	+·043

Table (continued).

Metal.	Surrounding Fluid.	<i>m.</i>	Voltaic constant.	E_H .	Thermovoltaiic constant.
SILVER	Sulphate	·042	1·539	1·890	—·351
	Nitrate	·25 to 2·0	1·495 to 1·556	1·890	—·395 to —·331
	Acetate	·057	1·489	?	{ Probably near to —·40
	Chloride: AgCl suspended in ZnCl ₂ solution.	·25 to 6·0	1·080 to 1·014	1·192 to 1·076	
	Chloride: AgCl suspended in CdCl ₂ solution.	·25	1·089	{ 1·192	{ —·103
	Chloride: AgCl suspended in CuCl ₂ solution.	·25	1·137		
	Bromide: AgBr suspended in ZnBr ₂ solution.	·25	·906	1·005	—·099
	Iodide: AgI suspended in ZnI ₂ solution.....	·25	·706	·726	—·020
LEAD	Sulphate: PbSO ₄ suspended in ZnSO ₄ solution.	·1 to 5·0	·536 to ·487	{ ·712 to ·704	{ —·176 to —·217
	Sulphate: PbSO ₄ suspended in CdSO ₄ solution.	·1 to 5·0	·550 to ·505		
	Nitrate	·25 to 2·0	·580 to ·591	·759 to ·716	—·179 to —·125
	Acetate	·25	·607	·770	—·163
	Chloride.....	·05	·591	·813	—·222
	Bromide.....	·037	·571	·806	—·235
	Iodide: PbI ₂ suspended in ZnI ₂ solution.....	·25	·455	·457	—·002
IRON	Sulphate.....	0·1 to 1·0	·432 to ·398	·284	+·148 to +·114
	Chloride.....	·25	·488	·284	+·204
	Bromide.....	·25	·418	·284	+·134
MERCURY ...	Sulphate: Hg ₂ SO ₄ suspended in ZnSO ₄ solution.	·1 to 5·75	1·457 to 1·514	?	{ Probably near +·25 to +·30
	Nitrate (mercurous).....	·25	1·499	1·202	
	Chloride: Hg ₂ Cl ₂ suspended in HgCl ₂ solution.	·25	1·257	{ ·668	{ +·589
	Chloride: Hg ₂ Cl ₂ suspended in CdCl ₂ solution.	·25	1·141		
	Chloride: Hg ₂ Cl ₂ suspended in ZnCl ₂ solution.	·25 to 9·5	1·123 to ·988	·668 to ·536	+·455 to +·481
	Bromide: Hg ₂ Br ₂ suspended in ZnBr ₂ solution	·25	·972	·500	+·472
	Iodide: Hg ₂ I ₂ suspended in ZnI ₂ solution.....	·25	·801	·267	+·534
MAGNESIUM.	Sulphate.....	1·0	—·725	—1·634	+·909
	Nitrate	·25	—·530	—1·631	+1·101
	Chloride.....	·25	—·701	—1·634	+·933
ALUMINIUM...	Sulphate.....	·5	+·537	—·982	+1·519
	Chloride.....	·25	+·280	—1·008	+1·288
	Bromide.....	·25	+·295	—1·008	+1·303

209. The following is a list of the cells examined, in which the value of $k_1 - k_2$ is opposite in sign to, and numerically greater than, E_H ; so that the current actually generated flows in the direction opposite to that predicable from the relative heats of formation of the electrolytes :—

Combination.	m .	E .	E_H .	$E - E_H = k_1 - k_2$.
Iron-cadmium-sulphate ...	0·1 to 1·0	-·073 to -·035	+ ·075	-·110 to -·148
Iron-cadmium-chloride ...	·25	-·157	+ ·082	- ·239
Iron-cadmium-bromide ...	·25	-·103	+ ·054	- ·157
Zinc-aluminium-sulphate ...	·5	+·537	- ·982	+1·519
Zinc-aluminium-chloride ...	·25	+·280	-1·008	+1·288
Zinc-aluminium-bromide ...	·25	+·296	-1·008	+1·304
Copper-silver-chloride. AgCl suspended in ZnCl ₂ solution	·25	-·020	} + ·154	{ - ·174
Copper-silver-chloride. AgCl suspended in CdCl ₂ solution	·25	-·010		
Mercury-silver-nitrate	·25	-·004	+ ·688	- ·692
Mercury-silver-chloride. Both suspended in ZnCl ₂	·25	-·046	+ ·525	- ·571
Mercury-silver-bromide. Both suspended in ZnBr ₂	·25	-·066	+ ·505	- ·571
Mercury-silver-iodide. Both suspended in ZnI ₂	·25	-·096	+ ·460	- ·556
Mercury-silver-chloride. AgCl suspended in ZnCl ₂ Hg ₂ Cl ₂ " CdCl ₂	·25	-·058	} + ·525	{ - ·583
Mercury-silver-chloride. AgCl suspended in CdCl ₂ Hg ₂ Cl ₂ " ZnCl ₂	·25	-·038		
Mercury-silver-chloride. Both suspended in CdCl ₂	·25	-·050		
Mercury-silver-chloride. AgCl suspended in ZnCl ₂ Hg ₂ Cl ₂ " HgCl ₂	·25	-·167		
Lead-mercury-chloride. Hg ₂ Cl ₂ suspended in CdCl ₂	·05	-·549	} + ·145	- ·694
Lead-mercury-chloride. Hg ₂ Cl ₂ suspended in ZnCl ₂	·05	-·539		
Lead-mercury-bromide. Hg ₂ Br ₂ suspended in ZnBr ₂	·037	-·402	+ ·306	- ·708
Lead-mercury-iodide. Both suspended in ZnI ₂	·025	-·347	+ ·190	- ·537

210. The following is a list of the cells examined in which the value of $k_1 - k_2$ is of the same sign as E_H ; so that the E.M.F. actually set up *exceeds* in numerical value that predicable from the relative heats of formation of the electrolytes; only those cells are named where the excess is somewhat considerable, *i. e.* 0·1 volt or upwards.

Combination.	<i>m.</i>	<i>E.</i>	<i>E_H.</i>	Excess.
Lead-copper-sulphate. PbSO ₄ suspended in ZnSO ₄ solution	0·1 to 2·0	·577 to ·611	} ·393 to ·394	{ +·184 to +·217 +·171 to +·202
Lead-copper-sulphate. PbSO ₄ suspended in CdSO ₄ solution.....	0·1 to 2·0	·564 to ·596		
Lead-copper-nitrate ..	·25 to 2·0	·486 to ·519	·345 to ·388	+·141 to +·128
Lead-copper-acetate	·25	·485	·341	+·144
Lead-copper-chloride	·05	{ Irregular, about ·475	·225	About +·25
Zinc-iron-sulphate	0·1 to 1·0	·445 to ·399	·284	+·161 to +·115
Zinc-iron chloride	·25	·490	·284	+·206
Zinc-iron-bromide	·25	·417	·284	+·133
Zinc-mercury-sulphate	·1 to 5·75	1·514 to 1·457	?	{ Probably about +·3
Zinc-mercury-nitrate	·25	1·500	1·202	+·298
Zinc-mercury-chloride. Hg ₂ Cl ₂ suspended in HgCl ₂ solution	·25	1·256	} ·668	{ +·588 +·572
Zinc-mercury-chloride. Hg ₂ Cl ₂ suspended in CdCl ₂ solution.....	·25	1·140		
Zinc-mercury-chloride. Hg ₂ Cl ₂ suspended in ZnCl ₂ solution	·25 to 9·5	1·123 to ·988	·668 to ·536	+·452 to +·481
Zinc-mercury-bromide. Hg ₂ Br ₂ suspended in ZnBr ₂ solution	·25	·972	·500	+·472
Zinc-mercury-iodide. Hg ₂ I ₂ suspended in ZnI ₂ solution.....	·25	·800	·267	+·533
Cadmium-mercury-chloride. Hg ₂ Cl ₂ suspended in HgCl ₂ solution	·25	·929	} ·302	{ +·627 +·510 +·497
Cadmium-mercury-chloride. Hg ₂ Cl ₂ suspended in CdCl ₂ solution	·25	·812		
Cadmium-mercury-chloride. Hg ₂ Cl ₂ suspended in ZnCl ₂ solution	·25	·799		
Copper-mercury-nitrate	·25	·433	·097	+·336
Lead-mercury-nitrate	·25	·917	·443	+·474
Lead-silver-chloride. AgCl suspended in ZnCl ₂ solution	·05	·489	} ·379	{ +·110 +·101
Lead-silver-chloride. AgCl suspended in PbCl ₂ solution.....	·05	·480		

211. The following is a list of the cells examined in which the value of $k_1 - k_2$ is of the opposite sign to, but not greater in magnitude than, E_H ; so that the E.M.F. actually set up is less in numerical value than that predicable from the relative heats of formation of the electrolytes; only those cells are

named where the deficiency is somewhat considerable, viz.
0·1 volt and upwards.

Combination.	<i>m.</i>	<i>E.</i>	<i>E_H.</i>	Deficiency.
Zinc-silver-sulphate	·042	1·536	1·890	·354
Zinc-silver-nitrate	·25 to 2·0	1·495 to 1·556	1·890	·395 to ·364
Zinc-silver-acetate	·057	1·490	?	Probably near ·40
Zinc-silver-chloride. AgCl suspended in ZnCl ₂ solution	·25 to 6·0	1·080 to 1·014	1·192 to 1·076	·116 to ·062
Zinc-silver-chloride. AgCl suspended in CdCl ₂ solution	·25	1·088	1·192	·104
Copper-silver-sulphate	·042	·4235	·785	·3615
Copper-silver-nitrate	·25 to 2·0	·429 to ·446	·785 to ·786	·356 to ·340
Copper-silver-acetate	·057	·397	?	Probably about ·39
Copper-silver-chloride. AgCl suspended in CuCl ₂ solution	·25	·035	·154	·119
Cadmium-silver-sulphate.....	·042	1·1805	1·533	·3525
Zinc-lead-sulphate. PbSO ₄ suspended in ZnSO ₄ solution	·1 to 5·0	·537 to ·487	} ·712 to ·704 {	} ·175 to ·217 {
Zinc-lead-sulphate. PbSO ₄ suspended in CdSO ₄ solution	·1 to 5·0	·550 to ·505		
Zinc-lead-nitrate	·25 to 2·0	·580 to ·591	·759 to ·716	·179 to ·125
Zinc-lead-acetate	·25	·608	·770	·162
Zinc-lead-chloride	·05	·591	·813	·222
Zinc-lead-bromide	·037	·571	·806	·235
Cadmium-lead-sulphate	·1 to 3·0	·173 to ·133	·355 to ·345	·182 to ·212
Cadmium-lead-chloride	·05	·260	·447	·187
Cadmium-lead-bromide	·037	·256	·468	·212
Lead-silver-sulphate. PbSO ₄ in ZnSO ₄ solution... }	·042	1·003	1·1775	·1745
Lead-silver-nitrate	·25 to 2·0	·914 to ·965	1·131 to 1·174	·217 to ·207
Lead-silver-acetate	·037	{ Irregular, about 0·8	?	Probably about ·3
Iron-lead-sulphate. PbSO ₄ suspended in ZnSO ₄ solution	·1 to 1·0	·120 to ·112	·428	·308 to ·316
Iron-copper-sulphate	·1 to 1·0	·685 to ·715	·821	·136 to ·106
Iron-silver-sulphate	·042	1·103	1·605	·502
Iron-silver-chloride. AgCl in ZnCl ₂ solution..... }	·25	·592	·908	·316
Zinc-magnesium-sulphate	1·0	·724	1·634	·910
Zinc-magnesium-nitrate	·25	·531	1·631	1·100
Zinc-magnesium-chloride	·25	·702	1·634	·932
Copper-magnesium-sulphate ...	1·0	1·840	2·739	·899
Copper-magnesium-nitrate	·25	1·595	2·736	1·141
Cadmium-magnesium-chloride .	·25	1·030	2·000	·970
Aluminium-copper-sulphate ...	·5	·578	2·037	1·509
Aluminium-cadmium-chloride .	·25	·050	1·374	1·324
Aluminium-cadmium-bromide .	·25	·022	1·346	1·324

212. The following is a list of the cells examined in which the value of $k_1 - k_2$ is not numerically greater than E_H , and lies within the limits ± 0.1 volt. It is evident that the number of cells coming into this category is only a fraction of the total number of combinations examined.

Combination.	<i>m.</i>	<i>E.</i>	E_H .	Differences.
Zinc-copper-sulphate	·25 to 2·25	{ 1·114 invariable	{ 1·105 invariable	{ +·009
Zinc-copper-nitrate	·25 to 8·0	1·066 to 1·091	1·105 to 1·100	+·005 to -·039
Zinc-copper-acetate	·25	1·091	1·111	-·020
Zinc-copper-chloride. Cu ₂ Cl ₂ suspended in CuCl ₂ sol., or CuCl ₂ sol. alone...	{ ·25	{ 1·0985	{	{ +·061
Zinc-copper-chloride. Cu ₂ Cl ₂ suspended in CdCl ₂ solution	{ ·25	{ 1·001	{ 1·038	{ -·037
Zinc-copper-chloride. Cu ₂ Cl ₂ suspended in ZnCl ₂ solution	{ ·25	{ ·988	{	{ -·050
Zinc-cadmium-sulphate	·1 to 5·0	·362 to ·357	·357 to ·367	+·005 to -·010
Zinc-cadmium-nitrate	·25	·351	·357	-·006
Zinc-cadmium-chloride	·25 to 8·0	·330 to ·262	·366 to ·240	-·036 to +·045
Zinc-cadmium-bromide	·25	·315	·338	-·023
Zinc-cadmium-iodide	·25	·322	·279	+·043
Copper-cadmium-sulphate	·1 to 2·0	·752 to ·755	·743 to ·748	+·009 to +·007
Copper-cadmium-nitrate	·25	·713	·748	-·035
Copper-cadmium-chloride. Cu ₂ Cl ₂ in CuCl ₂ sol., or CuCl ₂ sol. alone	{ ·25	{ ·769	{	{ +·097
Copper-cadmium-chloride. Cu ₂ Cl ₂ in CdCl ₂ solution	{ ·25	{ ·671	{ ·672	{ -·001
Copper-cadmium-chloride. Cu ₂ Cl ₂ in ZnCl ₂ solution	{ ·25	{ ·659	{	{ -·013
Zinc-silver-chloride. AgCl in CuCl ₂ solution ...	{ ·25	{ 1·136	{ 1·192	{ -·056
Zinc-silver-bromide. AgBr in ZnBr ₂ solution...	{ ·25	{ ·906	{ 1·005	{ -·099
Zinc-silver-iodide. AgI in ZnI ₂ solution	{ ·25	{ ·7055	{ ·726	{ -·0205
Cadmium-silver-chloride. AgCl in CuCl ₂ sol.	{ ·25	{ ·807	{	{ -·019
Cadmium-silver-chloride. AgCl in CdCl ₂ solution ...	{ ·25	{ ·761	{ ·826	{ -·065
Cadmium-silver-chloride. AgCl in ZnCl ₂ solution ...	{ ·25	{ ·751	{	{ -·075
Cadmium-silver iodide. AgI in ZnI ₂ solution	{ ·25	{ ·384	{ ·447	{ -·063
Zinc-lead-iodide. PbI ₂ in ZnI ₂ solution	{ ·25	{ ·455	{ ·457	{ -·002

XXIII. *On some Electromagnetic Experiments, continued.*—
 No. II. *Diverse views of Faraday, Ampère, and Weber.*
 By S. TOLVER PRESTON*.

IN Poggendorff's *Annalen* (1841, Bd. lii.) is a remarkable paper by Prof. W. Weber, entitled "Die unipolare Induction," which refers of course to the well-known fact of the induction of continuous currents by the influence of the *single* pole of a rotating magnet.

In spite of the comparatively old date of this paper, it is well known and referred to in the present day. This paper has some relation to the subject of my last communication to the *Philosophical Magazine* (February 1885); but I wish to point out that it does not alter or affect the conclusions there arrived at, but, on the contrary, tends to confirm them.

The paper of Prof. Weber appears to open out a new question, distinct from the phenomena dealt with in my communication; and I would call attention to the fact that Prof. Weber's idea would appear to involve one theoretic conclusion which appears at first sight improbable, but which, if true, would seem to render an experimental determination possible which might be capable of throwing some light on the physical nature of the electric "current." I am not aware that the subject has been considered from this point of view, and of course anything I submit will be open to criticism.

I will remark, first, that Prof. Weber agrees with my view (which accords with that of Ampère, and is opposite to that of Faraday†), viz. that the "lines of force," or field of force, about a magnet must be considered in that sense fixed or dependent on the magnet, that this field of force rotates when the magnet rotates on its axis, or, otherwise put—When a magnet rotates on its axis, the field of force must (in regard to the inductive effect) be considered as moving with the magnet and intersecting external conductors, in the same sense as it would do if the magnet were bodily *translated*.

The chief point which I wish to notice in Prof. Weber's paper is his view as to the cause of the rotation of a magnet

* Communicated by the Author.

† I may mention that the object of my last paper was to point out an apparent ambiguous case (involving an oversight) in Faraday's experiments, which caused him to take the opposite of the above view as to the *rotation of the field of force with the magnet*, whereby Faraday contradicted Ampère's theory in this respect, although he speaks in approving terms of the theory in other respects.

on its axis, when one terminal of a voltaic battery is maintained in sliding contact with the equatorial part of the magnet, the other terminal in sliding contact with the pole of the magnet (a perfectly well-known experiment).

Prof. Weber thinks that, because in this case the current flows during part of its course *within the body of the magnet*, the explanation of the rotation afforded by Ampère's theory is "*not at all*" (*keineswegs*) applicable*.

Prof. Weber says (translation):—"The two effects (*i. e.* the rotation of a magnet on its axis, and the converse case of the rotation of a conductor about the magnet), which Ampère here endeavours to explain from *like* causes, are, however, in reality of different nature; and each demands its own explanation. The explanation given by him (Ampère) only applies to the rotation of the conductor about the magnet, discovered by Faraday, and not at all to the rotation of the magnet on its axis discovered by himself."†

But now I would call attention to the fact that the rotation of the magnet is found to take place apparently equally well when the current *does not enter the body of the magnet at all*, such as when the magnet is coated with a metallic sheath or cylinder, insulated from the body of the magnet by intervening cement or paper, so that the current flows along the sheath instead of entering the body of the magnet.

If, then, the magnet rotates in this case (when no current enters its body), how, it may be asked, can it be said that Ampère's explanation of the rotation when the body of the magnet (instead of the metallic sheath) is used to conduct the current, is "*not at all*" (*keineswegs*) applicable?

For Ampère's theory, when the magnet is coated with the sheath, *admittedly* accounts for a force of rotation of the magnet (on its axis) *equal* to that with which (conversely) the external loop—called for brevity "conductor"—would rotate about the magnet if it (the loop or "conductor") were free to move. Ampère's theory therefore accounts for a force of rotation in the magnet equal to the opposite force of

* Ampère, as is known, considered the rotation of the magnet on its axis to be due to the *same* cause as the (converse) rotation of the "conductor" (or external loop of wire) about the magnet, or he considered the effect to be a simple case of "action and reaction" between the movable parts of the circuit; *i. e.* whether the conductor revolved about the magnet, or (conversely) the magnet rotated on its axis, depended simply on which was free to move. The movement (on Ampère's view) depended on the reaction between the current in the conductor and the *external* magnetic field of the magnet, whereby the two tended to be twisted or to rotate in opposite directions.

† Pogg. *Ann.* 1841, Bd. lii. S. 354.

rotation of the conductor about the magnet. What more, may I ask, do we want, or is not this sufficient? Is it to be supposed that when the body of the magnet conducts the current instead of the insulated sheath, the force of rotation of the magnet is then (by some action of the portion of the current within the magnet) *greater* than in the former case, *i. e.* greater than the (converse) force of rotation of the conductor about the magnet? In other words, when (in the first case) the magnet is coated with a sheath (simply to prevent the current from entering its body) the rotative forces of the magnet and conductor are equal: then (in the second case), when the sheath is removed, is it to be assumed, or does Prof. Weber's theory imply, that the opposite rotative forces are unequal (so that the magnet and the conductor now tend to revolve in opposite directions about each other with unequal forces)? It appears that Ampère's theory is sufficient to account for *equal* rotative forces in magnet and conductor in opposite directions. Does Prof. Weber's view imply that *unequal* forces of torsion—or of tendency to rotation—in opposite directions have to be accounted for, or exist *when the current flows into the body of the magnet*?

If so, a curious result would appear to follow, viz. (the opposite moments of torsion being assumed unequal, or the moment of torsion of the magnet on its axis being assumed *greater* than that of the conductor about the magnet) a resultant moment of torsion must exist in the system ending to turn it about its centre of gravity. Or otherwise expressed, it would follow apparently that if the whole system, with a small voltaic battery fixed in it, were all delicately suspended as one whole, it would tend to rotate about its centre of gravity by the action of forces within itself without any external visible support or point of reaction.

I would call attention to this improbable result, which appears to follow from Prof. Weber's view. But I am not prepared to contend that this result might not be *possible* under certain conceptions. If found to be true on trying the experiment, the result would be highly interesting, as its analysis would probably be capable of throwing a light on the physical nature of the electric "current."

Another way of trying the experiment might suggest itself. The moment of torsion of the magnet on its axis might be exactly measured, first with the insulated sheath, and, secondly, without it. If in the last case (*i. e.* without the sheath), the moment of torsion were found to be greater, this would (as stated) appear to indicate the existence of a resultant moment of torsion which, when the sheath is removed, tends to twist

the whole system—conductor, magnet, battery and all—about a common centre of gravity. This result (as remarked) will appear improbable at first sight at least. If found to be confirmed on making the experiment, it would not, as it seems to me (and I would put this as a *query*) necessarily invalidate Ampère's theory, which accounts for *equal* forces of rotation in opposite directions. It would rather add an additional or new experimental fact to knowledge.

P.S. The conditions stated in the above paper are, I think, capable of settling this question; and in this I am confirmed by a letter recently received from Prof. Wilhelm Weber, of Göttingen, to whom the present manuscript was previously sent. I quote the following remark from Prof. Weber's letter (dated February 15, 1885):—

“Ich habe sie (*i. e.* the present manuscript) mit grösstem Interesse gelesen und mich gefreut, dass die Frage über *unipolare Induction* dadurch endlich zu definitiver Erledigung gelangen werde.”

I may say I had, some time ago, received a letter from Lord Rayleigh, in which he spoke favourably of my view in the last paper (*Phil. Mag.*, Feb.), which was in opposition to that of Faraday. I pointed out in my last paper that the experiment on which Faraday founded his view was capable of a *double meaning*. The subject appears an extremely interesting one to follow out experimentally, as there are conditions in the inquiry which would seem, if investigated, to be capable of throwing a light on the *physical nature* of the electric “current,” and even of magnetism.

Paris, February 1885.

XXIV. *On Prof. Edlund's Theory that a Vacuum is a Conductor of Electricity.*

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

I SHOULD like to call the attention of Prof. Edlund to an objection to his view that a discharge-resisting vacuum is nevertheless a conductor of electricity. The objection is one that I urged in a letter to ‘Nature’ in the spring of 1883, and, so far as I am aware, it has not yet been answered. I refer to the fact that a body surrounded by such a vacuum is acted upon inductively by another electrified body near it, and not screened from the action as it would be if surrounded by a conductor.

Although there was probably when I wrote already sufficient experimental evidence to show that induction took place across such a vacuum, yet I thought it worth while to make the experiment described in my letter, in which a platinum ball suspended in such a vacuum is shown to be strongly attracted towards a neighbouring electrified platinum sheath, precisely as if suspended in any other fluid dielectric, while a minute spark was seen at the moment when the two attracting bodies came into contact.

This tube and others that I experimented with exhibited the luminous effects described by Prof. Edlund when moved in the neighbourhood of an electrified body or influenced by the passage of currents in the neighbourhood; but I do not think that this luminosity is necessarily the proof of a current in the ordinary sense. It may be due to the increase or diminution of induction within the medium, some of the energy of the induction-tubes being absorbed in transit.

It would be interesting to make the experiment of passing a strong current through a stout wire surrounded by such a vacuum, and to observe whether the transit of energy across the vacuum* was attended by any luminous effect. If it were, then the vacuum might be regarded as a conductor in so far as the inductive energy entering is transformed into the luminous form. If no effect were observed, we should infer that this transformation did not take place when a steady flow was reached, but only accompanied variations in the flow.

I am your obedient servant,

Montreux, February 14, 1885.

A. M. WORTHINGTON.

XXV. *On the Application of the Pendulum to the Determination of the Mean Density of the Earth.* By Dr. J. WILSING, of Potsdam †.

THE first experimental determination of the mean density of the Earth which Maskelyne and Hutton carried out depends on the observation of deflection of the plumb-line in the neighbourhood of Schehallien. It is limited by the uncertainty arising from our imperfect knowledge of the mass and density of the deflecting mountain, an error which also affects the results deduced by Carlini and Airy from oscillations of the pendulum. Accordingly the use of the pendulum has been abandoned in favour of the torsion-balance, as it is not sensi-

* See Prof. Poynting's paper "On the Transfer of Energy in the Electromagnetic Field," *Phil. Trans.*, Part II. 1884.

† Translated from the *Sitzungsberichte der Berliner Akad. der Wissenschaften*, January 1885.

tive enough to be appreciably influenced by smaller masses which are capable of accurate determination.

The necessary degree of sensitiveness may be obtained by adjusting the centre of gravity of the pendulum close under the axis of oscillation. With an experimental apparatus, which was intended to get some indications as to the practical execution of this modification, experiments were made in September of last year in the observatory. This consisted of a prismatic rod of thin sheet-iron, at the ends of which were fixed lead balls, each weighing 300 grammes. The steel knife-edge, which worked on agate, was fixed in the middle of the rod. The apparatus was so arranged that, when in equilibrium, its axis was almost vertical. The motion was read off by reflection. When the position of equilibrium was found and the attracting masses were placed near the lead spheres, from the deflection of the pendulum, the ratio of their attraction to the constant of gravitation can be determined. By reversing the direction of the deflecting forces, the action of the force can be doubled.

This method may be regarded as a combination of the two oldest methods. In comparison with the method used by Prof. von Jolly with such distinguished success, it has the advantage that the sensitiveness of the pendulum is considerably greater.

Although the crude construction of the experimental instrument made it very difficult to introduce the needful corrections,—it being mounted on a wooden support, and not sufficiently protected against currents of air,—a time of oscillation of 150 seconds could be attained without the occurrence of irregularities which could be regarded as essential. This may be explained by the fact that partial oscillations are excluded as completely as possible, and the axis of the pendulum deflects but a little on each side of the vertical, so that the bending of the rod is only a very slight amount. The degree of delicacy corresponding to the above-mentioned time of oscillation would be sufficient to measure the attraction of spheres of a few hundredweight, as is possible by using a torsion-balance.

The sensitiveness of the apparatus was ascertained from the observation of oscillation by the addition of weights, which were placed at known distances from the knife-edge at the lower end of the pendulum. If gm is the weight of the piece added, d the distance of its centre of gravity from the knife-edge, the time of vibration for infinitely small oscillations is

$$T = \pi \sqrt{\frac{MK^2 + mk^2 + md^2}{g(Ms + md)}}.$$

From this, in connection with the time of vibration of the unloaded pendulum, we get the magnitude gMs , which forms the denominator in the expression for the angle of deflection,

$$\tan \phi = \frac{g'ml}{gMs},$$

where g' is the attraction of the deflecting mass, m the mass of the pendulum-bob, l the distance of its centre from the knife-edge. Closer consideration shows that the use of this method is even allowable if a correction is made for the buoyancy of the air, Bessel's correction for the moment of inertia, and for a possible blunting of the knife-edge; for the form of the equation does not alter, and only the magnitudes gMs and MK^2 acquire a different signification.

As an example of the determination I may here give the results of two series of observations. In the first the duration of the free oscillation was:—

		$T_0 = 59\cdot20^s$
Extra weight 1 gr.	. . .	$T_1 = 22\cdot00$
„ „ 5 gr.	. . .	$T_5 = 10\cdot46$
„ „ 10 gr.	. . .	$T_{10} = 7\cdot48$

Expressed in grammes and millimetres, we have for gMs the values

$$81\cdot96, \quad 81\cdot25, \quad 81\cdot89.$$

In the second series the time of the free oscillation was:—

		$T_0 = 145\cdot20^s$
Extra weight 1 gr.	. . .	$T_1 = 23\cdot64$
„ „ 3 gr.	. . .	$T_3 = 13\cdot66$
„ „ 8 gr.	. . .	$T_8 = 8\cdot41$

and for gMs ,

$$13\cdot75, \quad 13\cdot50, \quad 13\cdot50.$$

The concordance between these numbers, which are independent of each other, is also somewhat limited by the error of the extra weight, the amount of which was not known. The experiments show, however, with certainty that the value of the reduction may be determined with great accuracy with an apparatus of convenient construction.

Besides the calculation of the gravitation of the pendulum-bob towards the attracting masses which is common to all methods, two measurements are necessary—the distance of the extra weight, and of the centre of the pendulum-bobs from the knife-edge. These measurements, however, require no other appliances than those which are in use with the reversion-pendulum.

The result of the experiments hitherto made renders it scarcely doubtful that the pendulum, which does such excellent service in all problems bearing on the determination of gravity, will also give useful results for our present purpose; and in the immediate future I hope, by means of an apparatus constructed by Repsold, of Hamburg, to continue these observations in the astrophysical observatory and to bring them to a conclusion.

XXVI. Notices respecting New Books.

A Treatise on the Principles of Chemistry. By M. M. PATTISON MUIR, M.A., F.R.S.E., Fellow, and Prelector in Chemistry, of Gonville and Caius College, Cambridge. Cambridge: The University Press.

MR. MUIR'S book is certainly one of the most important contributions which have been made to general English Chemical literature for many years, and will, we feel sure, be heartily welcomed by all who are engaged in teaching chemical science in this country. A text-book of a like scope to Lothar Meyer's well-known *Moderne Theorien der Chemie* has long been a desideratum; and it is surprising that a translation of the latter into the English language has not been published long ere this. Such a translation will, however, not be in such request now that Mr. Muir has brought out a book similar in kind, and, as we think, of equal merit, and probably more suitable for English students than a translation of the German work would have been.

The volume is not of course an elementary text-book, but is intended for advanced students and teachers, giving a lucid and comprehensive view of chemical theory and the general principles of the science down to the most recent date. The author says he has "tried to deal with chemical facts and generalizations so as to show their reality," and in this we think he has eminently succeeded. Mr. Muir possesses the happy method of marshalling even dry facts in a manner both interesting and instructive, and of eliciting therefrom the information which they are designed to teach. His success in this respect is in great part due to his having throughout "followed in the very footprints of the great discoverers, watched them as they make their footing sure and as they feel their way up the heights." This is undoubtedly the plan on which a treatise on chemical principles and theory should be written. When presented in this manner the student is always in a position to see where fact ends and hypothesis begins, and is thus enabled to judge for himself as to the particular value to be placed on any given theory. As an example of this we may mention more especially the capital summary of atomic heat-determinations and their bearing on the determination of atomic weights. This method has also an educational value in showing how abstruse chemical

problems are worked out and made capable of presentation in a simple and concise manner.

The work is divided into two parts. In the first, which in the main deals with Chemical Statics, we have a statement and discussion of the Atomic and Molecular theory, and its application to such subjects as isomerism, allotropy, nascent action, and classification. In this part there is also an account of the application of physical methods to the solution of chemical problems. The second part treats with such subjects as may be conveniently considered under Chemical Kinetics, as dissociation, chemical change, and equilibrium, affinity, and the relations between chemical action and the distribution of the energy of the changing system.

The chapter on Atoms and Molecules is remarkably well written, and gives a clear account of the growth of the views at present held by chemists. In that portion dealing with the molecular weights of solids, the author seems to have overlooked the important work of Prof. Louis Henry on the Polymerization of the Oxides, in which this subject is treated in a very masterly manner and most cogent arguments adduced in favour of the view that the molecular weights of solids are much higher than is represented by the simple formulæ usually assigned to them. Our own researches on the colour of chemical compounds, so far as they go, are in favour of Prof. Henry's views.

After an account of the service which vapour-density, specific heat, and isomorphism render in the determination of atomic weights, the nature of the methods based on purely chemical evidence is next described; and the chapter finally concludes with an exceedingly good and useful table giving a summary of the most important facts concerning the atomic weights of the elements. This table we consider to be a marked and very valuable feature in the book; for we fully agree with the author that "it is well that the student should have placed before him a synopsis of the evidence on which these all-important numbers are based." The table is also rendered additionally useful by copious references to the original memoirs.

Chapter II gives a good résumé of the more important facts bearing on "nascent action," an expression which the author considers to have been "at once helpful and harmful to the progress of Chemistry. By classing under a common name many phenomena that might otherwise have been lost in the mass of facts with which the science has to deal, the expression has done good service; but in so far as its use has tended to prevent investigation—for it is always easier to say of any unusual reactions, These are cases of nascent action, than to examine carefully into their cause and conditions—the use of the expression has been unfavourable to the best interests of chemical science." The sections of this chapter treating of equivalency, allotropy, and isomerism are well worthy of careful perusal, and will undoubtedly serve a good purpose in giving teachers of Chemistry a clearer and more concise idea of the meaning which *ought* to be attached to the vexed term "atomicity." The

view supported by Mr. Muir is that put forward more especially by Lossen; and from the arguments adduced this seems to be the most philosophical in the present state of our knowledge.

The section on Isomerism includes an account of Groth's crystallographical researches on the derivatives of benzene, and gives in detail the more important inexplicable phenomena of isomerism presented by hydrobenzoin, dulcitol, the lactic and tartaric acids, &c., the discussion of which adds considerably to the interest of this portion of the book. Here also Lehmann's important researches on physical isomerism have a prominent place.

The Periodic Law is treated in a separate chapter, and more fully, we believe, than in any other English text-book. It will thus be of valuable assistance to those who are unable to consult the original memoirs on this subject. Scarcely sufficient credit, however, is given to Newlands, who, in the publication of his 'Law of Octaves,' was certainly the first to draw attention to the periodic connection between the atomic weights and the properties of the elements. The periodic relation between the atomic weights and the heats of formation of the elements with chlorine, bromine, and iodine, which was first pointed out by the writer of this review in a paper published in the 'Proceedings' of the Royal Society, and also at greater length in another paper in the *Chemiker Zeitung*, is erroneously ascribed by Mr. Muir to Laurie, whose paper was only published by the Royal Society of Edinburgh some years subsequently.

The section dealing with Thermo-chemistry will, we think, be of very great service to teachers, as there are but few English text-books which even refer to this subject. The value of this section is also greatly enhanced by the numerous, varied, and well-chosen examples which are given of problems dealing with heat-changes produced by chemical action.

In Book II., under the head of Chemical Kinetics are included facts and principles which chiefly relate to chemical action as opposed to those which refer more especially to chemical composition. It is evident, as the author states, that this division can only be carried out in the broadest way, but it is nevertheless a very convenient and useful division for working purposes. The same facts, for a complete knowledge of their bearings, must be studied from both a Statical and Kinetical point of view.

Under Kinetics we have a chapter on Dissociation, followed by one on Chemical Change, wherein are discussed at length the theories of Williamson, Pfaundler, Horstman, and Gibbs; and an account is also given of the work of Gladstone, Harcourt, and Menshutkin on this subject. In a chapter on Chemical Affinity much stress is laid on the dynamical hypothesis of Guldberg and Waage, which is shown to be almost independent of any molecular theory of the structure of matter; whilst, as extended by Ostwald, "it forms a bridge connecting the investigation of the chemical properties of molecules with that of the action of the forces which come into

play during chemical operations. The thermo-dynamical methods of investigation introduced by Horstman, Gibbs, and others, and the electrical methods founded on the work of Joule and Thomson, and developed by Helmholtz and Wright, also enable us to gain some conceptions of the conditions under which chemical changes proceed and chemical equilibrium is established, and at the same time throw a little light on the most profound parts of chemical phenomena, the nature and conditions of action of the forces concerned in the combinations and decompositions of atoms."

The fair and cautious manner in which the author introduces all the ordinarily accepted theories, as well as those which are less known, is one of the excellent points of the book; whilst his parting advice, "to exhibit the hypotheses of Chemistry as at once arising from facts and serving as guides in the quest of facts," is what all teachers and workers in our science will do well to bear in mind, remembering that one of the chief difficulties in "the use of chemical hypotheses consists in determining the limits of the class of phenomena to which each hypothesis can be applied."

In conclusion, we can commend Mr. Muir's book most heartily to both teachers and taught as a reliable source of information on general chemical theory as well as on many points not easily found in the ordinary text-books, and the accounts of which as occurring in the original memoirs are generally in a too unwieldy condition for reference when time and opportunity are limited. We believe that no one will rise from a perusal of this book without feeling that his mind has become cleared on many points and his sympathies widened into taking a much deeper interest in those branches of the science with which he is not immediately concerned, whilst many will experience an increased desire to have a share in the solving of problems which have not yet yielded to the force of investigation. As page after page is passed in review, we feel that we are indeed following in the "very footprints of the discoverers."

THOS. CARNELLEY.

XXVII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 148.]

January 28, 1885.—Prof. T. G. Bonney, D.Sc., F.R.S., President, in the Chair.

THE following communications were read:—

1. "The Boulder-clays of Lincolnshire: their Geographical Range and Relative Age." By A. J. Jukes-Browne, Esq., B.A., F.G.S.

The author commenced by referring to the late Mr. Searles V. Wood's papers on the Glacial beds of Yorkshire and Lincolnshire, and stated, as the result of his own investigations, that two distinct types of Boulder-clay occur in Lincolnshire, (1) the grey or blue

clay, (2) the red and brown clays; the former undoubtedly an extension of the Upper or Chalky Boulder-clay of Rutland and East Anglia, while the second includes the Purple and Hessle Clays of Mr. S. V. Wood. These two types of Boulder-clay are very rarely in contact with each other.

The brown Boulder-clays of East Lincolnshire rest upon a broad plain of Chalk, which appears to terminate westward in a concealed line of cliff, this cliff-line coinciding with the strike of the slope which descends from the Chalk Wolds to the Boulder-clay plateau by which they are bordered. The present boundary-line of the Boulder-clay runs along this slope for long distances, though in many places the clay has surmounted the slope and caps the hills to the west of it.

From Louth the main mass of the "brown clay" is bounded by a line drawn through Wyham, Hawerby, Laceby, and Brocklesby to Barrow and Barton on Humber, sweeping round the north end of the Lincolnshire Wolds and occurring on both sides of the Humber. Previously to the author's inspection of this district no Purple or Hessle clay had been discovered west of South Ferriby, and these clays were supposed to be entirely absent on the western side of the Wolds. The officers of the Survey have, however, mapped several tracts of such clay in the valley of the Ancholme. It occupies the surface at Horkstow, Winterton Holme, Winterton, and Winteringham. It probably underlies the alluvium of the Ancholme near and south of these places, and occurs again at higher levels in the neighbourhood of Brigg. South of Brigg it has been seen at low levels on either side of the valley of the Ancholme, as far as Bishop's Bridge near Glentham.

Beyond this point it was not traceable in the Ancholme valley, but south of Market Rasen patches of reddish-brown clay, mottled with grey, and containing small flints and pebbles of chalk, occur, and cap the low ridges separating the valleys of the brooks.

Another tract of Boulder-clay, which the author considers to belong to the same series, occupies the western border of the Fenland S.E. of Lincoln, what is left of it forming a ridge which runs southward for many miles. It passes eastward beneath the Fen deposits; and similar mottled clay was seen in the excavations for the Boston docks beneath about 20 feet of Fen-clays &c., and resting upon blue Boulder-clay of the "Chalky" type. Besides this section at Boston, there are very few places where the two types of clay are in contact, or so near as to afford any evidence as to their relative age. Near East and West Real, and again near Louth, the "Brown Clays" are banked against the slopes of hills which are capped with the "Chalky Clay." The same is the case also near Brigg, where the country seems to have been originally covered by a sheet of the Chalky Clay, through which valleys were eroded into the Jurassic clays, and the brown (Hessle) clay is found only in these valleys.

The author concludes, therefore, that the "Brown-clay series" is of much newer date than the "Blue and grey series."

In conclusion, the author summed up the inferences drawn in the paper, correlated the Basement Clay of Holderness with the Chalky Clay of Lincolnshire, and suggested that the Purple Clay may be confined to the east side of the wolds. The classification he would propose is therefore as follows:—

	Lincolnshire.	Yorkshire.
Newer Glacial.	{ Hessle clay.	Hessle and upper red clay of coast.
	{ Purple clay.	Purple clay.
Older Glacial =	Chalky clay.	Basement clay.

2. "On the Geology of the Rio-Tinto Mines, with some general Remarks on the Pyritic Region of the Sierra Morena." By J. H. Collins, Esq., F.G.S.

After briefly describing the geographical position of the Rio-Tinto mines and the occurrence at the same of pyritous ores amongst slates and schists which abut against gneissose rocks to the north, and pass under Tertiary beds to the southward, the author proceeded to consider the general characters and associations of the pyrites-deposits, and then gave a general account of the Rio-Tinto district. The slates were described, and the fossil evidence recapitulated upon which an Upper Devonian age had been assigned to them. Analyses were furnished to show the changes due to weathering and to infiltration. The various intrusive rocks (syenite, diabase, and porphyries) occurring in the schists were described, and analyses of them given. The sedimentary iron-ores and their composition were next noticed, and the author ascribed their formation to deposition in lakes.

The masses of pyrites which furnish the principal ores of Rio Tinto were then described, their mode of occurrence in fissures between dissimilar rocks explained, and their formation discussed. The different kinds of ore obtained from the mines were noticed in detail, and several analyses added, giving samples both of the mixed ores and of the pure minerals.

The manganese-lodes were next described, and shown to be parallel to the pyrites-fissures, and frequently to be only branches of the latter.

A summary of the author's conclusions as to the stratigraphy of the district, the ore-deposits, and the surface-geology was appended.

3. "On some new or imperfectly known Madreporaria from the Great Oolite of the Counties of Oxford, Gloucester, and Somerset." By R. F. Tomes, Esq., F.G.S.

February 11.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., President, in the Chair.

The following communications were read :—

1. "The Tertiary and Older Peridotites of Scotland." By John W. Judd, F.R.S., Sec. G.S.

The very interesting rocks known as "peridotites" have been regarded by many petrographers as peculiar to, and, indeed, characteristic of, the older geological periods; but in the Western Isles of Scotland there occur a number of rocks of this class, constituting portions of intrusive masses, which the author, in a previous paper, has shown to be the central cores of Tertiary volcanoes of vast dimensions.

These Tertiary peridotites are most intimately associated with the gabbros and dolerites, the felspathic and non-felspathic rocks passing into one another by insensible gradations, and the rocks of either class being intersected by veins of the other. The peridotites exhibit the same varieties of microscopic structure as the associated gabbros and dolerites, these structures being described under the names of "granitic," "ophitic," and "porphyro-granulitic."

The felspars, which are rare in the peridotites, are intermediate in composition between labradorite and anorthite; they rarely, however, exhibit evidence under the microscope of being built up of laminae belonging to different species. The study of the lamellar twinning, which is a common, but by no means universal, character in these felspar crystals, points to the conclusion that it has been induced by pressure or strain, like the similar structure in rock-forming calcite. The pyroxenes are represented by many varieties, both of the monoclinic forms (augites) and the rhombic forms (enstatites), the former being by far the most abundant. The olivines below are, for the most part, highly ferruginous varieties. The spinellids, magnetite, chromite, and picotite occur in these rocks, as do also titano-ferrite and its alteration-products. Among the accessory constituents biotite is the most abundant.

It was shown that each of the minerals of these rocks is found to undergo remarkable changes as we pass from the superficial to the central portions of these intrusive rock-masses. The most important of these changes is that for which the author proposed the name "schillerization." It consists in the development of microscopic enclosures, in the form of plates and rods, along certain planes within the crystal, giving rise to metallic reflections or a play of colour. The felspars, pyroxenes, and olivines are all found to be affected in this way when they have formed the deepest parts of these volcanic cones. In this way common augite is seen at gradually increasing depths, passing into the deep brown variety known as pseudo-hypersthene. The last-mentioned substance presents a curious "mimicry" of true hypersthene and paulite, which is the schillerized form of a ferri-ferous enstatite.

The Tertiary peridotites present many variations, not only in

their structure but also in their mineralogical constitution. Among them occur examples of the rocks which have received the names of dunite, pierite, and lherzolite, with some curious types composed of feldspar and olivine.

Among the older peridotites of Scotland a new and very interesting type is described from near Loch Seye in Caithness. It appears to have been originally a mica-pierite, but the whole of the original minerals have been converted into paramorphs, firstly by schillerization and subsequently by amphibolization and serpentization.

In conclusion, it was pointed out that the discrimination between the effects of the changes described as schillerization and those known as uralitization, amphibolization, serpentization, and kaolinization is of the utmost importance, not only to the petrographer but to the mineralogist.

2. "Boulders wedged in the Falls of the Cynfael, Ffestiniog." By T. Mellard Reade, Esq., F.G.S.

This paper briefly described certain phenomena of stream-denudation observed in the bed of the Cynfael, which has cut a deep channel through the Lingula Flags, the course of the channel being mainly dependent on the jointing of the rock. In one spot the upper beds at the top of the gorge have slid upon the lower along their dip, about 10° to north by east, so as to project over the stream like a corbel; and advantage has been taken of this to form a bridge by means of a slab of rock laid from the projecting mass to the top of the opposite bank. At another point several very large boulders are stuck fast in the channel, and the stream flows beneath them. The boulders could not possibly have been carried down the existing gorge, and they had not, the author believed, fallen from above. He suggested that they might have been carried down by the aid of ice, probably in the glacial period, when the stream ran in a wider channel, and that they had been polished by the action of the water.

XXVIII. *Intelligence and Miscellaneous Articles.*

de COMBINATIONS OF SILVER CHLORIDE, BROMIDE, AND IODIDE
OR WITH COLOURING-MATTERS. BY M. CARY LEA, PHILADELPHIA.

WHILE studying these silver-salts in May last, I found that they had the remarkable property of entering into chemical combination with many colouring-matters very much in the same way that alumina does, though not to the same extent, forming what may be called lakes. It is only necessary to bring freshly precipitated and still moist silver-salt into contact with the colouring-matter, or to make the precipitation in the presence of the colouring-matter if the latter is not precipitated by silver nitrate, when the combination takes place and the colouring-matter cannot be washed out by any amount of washing. A prolonged absence following immediately after has prevented the continuation of the investigation.

It is still incomplete, and the leading facts only are mentioned here to take date.

Not all colouring-matters are capable of uniting with the silver-salts, but the number of those that do so unite is considerable. What is curious and tends to show that the combination is intimate, is that the colour assumed by the silver-salt is not always that of the dye, but may differ from it considerably. Also the three silver-salts may be differently coloured by one and the same colouring-matter.

More frequently, however, colouring-matters impart their own shade or something approaching to it. Thus, silver bromide precipitated in presence of excess of silver nitrate takes from aniline-purple a strong purple colour; from cardinal-red a bright flesh or salmon-colour; from naphthaline-yellow a light yellow colour; from eosin a brilliant pinkish or salmon; and so on.

Different specimens of the same colour gave sometimes quite different results: thus, silver bromide precipitated in presence of silver nitrate was dyed by one specimen of methyl-green to a bluish green. Another specimen of the same colour obtained from a different source coloured the same silver-salt a deep purplish shade. Silver iodide showed the same difference.

Sixteen years ago I proposed to colour or stain the photographic film in order to modify its behaviour towards light, principally to prevent blurring or irradiation*.

Of many colouring-matters then tried, the best proved to be litmus coloured red by acetic acid. This was very effectual for the purpose, and was long used by others as well as myself. So far as I have been able to ascertain, it was the first suggestion made of this mode of acting on the sensitive plate. Since then, staining the film has been found to have other applications; and many others have experimented in this direction, in most cases with a view to alter its sensitiveness relatively to the different colours of the spectrum. Major Waterhouse was, I believe, the first to recognize this effect.

Dr. John W. Draper appears to have first advanced the view that substances sensitive to light are affected by the rays which they absorb. There is much to support this theory, although it cannot be considered as definitely established.

Some years since Dr. H. W. Vogel expressed the opinion that, when sensitive films were washed over with solutions of colouring-matters, the films gained sensitiveness to those rays of the spectrum which the colouring-matters absorb, with this condition—that the colouring-matter in question must be capable of combining with Cl, Br, or I, as the case might be. My own results were different. I found that the action of the rays was profoundly modified by colouring the film; but the result did not seem to conform to any law, and as often contradicted Vogel's view as agreed with it.

Vogel's theory necessitates the assumption that the colour

* *British Journal of Photography*, 1868, pp. 210, 506; 1870, p. 145 &c.

imparted to the silver-salt is identical with that of the dye used; and, as has been shown above, that by no means follows. He supposed that the capacity of any given colour to influence the silver-salt depended upon its tendency to combine with Cl, Br, and I; whereas, as we have above seen, its action most probably depends upon its ability or inability to combine with the silver haloid.

But the principal source of error has arisen from the fact that when the film is stained, the effect is necessarily a confused one. Besides any influence that may be exerted on the particles of silver haloid, these particles are virtually behind a colour-screen, which must materially modify the nature of the light that reaches them, and the final effect must necessarily be a combination from two distinct causes. Moreover, the colour in the film tends to arrest precisely those rays to which it is proposed to render the silver-salt more sensitive; a consideration of the utmost importance, for the one action tends to counteract the other, and thus leads to inextricable confusion. From a system of experiments so faulty no just conclusions could be drawn.

Whether, with these sources of error eliminated, Draper's view, that a sensitive substance is influenced by those rays which it absorbs, can be applied to these new combinations which I have here described, is a matter on which I am not prepared to express an opinion, having been, much to my regret, unable as yet to examine the question. It seems *à priori* probable; but in that case it is important to observe that *the effect will depend, firstly, upon the capacity of the dye to combine with the silver haloid, and, secondly, not on the proper colour of the dye isolated, but on the colour that the silver haloid acquires under its action*, which, as we have already seen, may be something quite different from the colour of the original dye.

I have observed that the silver-salts are greatly changed by conversion into lakes, even when the colour imparted is but faint. They become in some cases much more finely divided and remain long in suspension. In one case at least a great increase of sensitiveness to light for development was observed. Later I shall hope to give more definite details on these points.

In the above facts will doubtless be found an explanation of many of the anomalies in the behaviour of coloured films which have caused such wide differences of opinion. And the new modes of operating deducible from the reactions here described will, I think, be found of extended utility. Silver-salts can be dyed first and emulsified afterwards; and the ability to colour the sensitive salt to any shade with certainty, and without introducing a counter-acting influence into the film, gives a new power in photochemistry. —Silliman's *American Journal*, January 1885.

ON A SELENIUM-ACTINOMETER. BY M. H. MORIZE,
OF RIO JANEIRO.

The object of this instrument is to measure the relative intensities of solar luminous rays at different heights on the horizon.

The selenium-actinometer consists of a cylinder of selenium prepared on Mr. Graham Bell's plan. Thirty-eight disks of copper are isolated from each other by disks of mica of smaller radius; the groove thus formed is filled with selenium by being rubbed with a rod of this substance. The cylinder being suitably heated, this selenium acquires a greyish aspect, and is ready for use. The even numbers of copper are united by conductors, and the odd numbers also by another set of conductors. By this arrangement not merely is the resistance of the selenium lessened, but we may still increase the delicacy of the apparatus by increasing the number of disks and that of the layers of selenium, while diminishing the resistance of the latter.

The selenium cylinder is insulated by glass supports in the interior of a glass cylinder, which is exhausted so as to protect it from the disturbing influence of obscure heat.

The whole is placed on a support high enough to avoid the effects of light reflected from adjacent objects. In placing the cylinder care is taken to do it so that its axis is parallel to the terrestrial axis. In this way the sun's rays strike the selenium almost perpendicularly at any hour of the day and always illuminate the same portion. By a slight motion in the plane of the meridian the cylinder could each day be brought into such a position that the rays are quite perpendicular.

If, now, we pass a constant current through this apparatus and through a galvanometer, the copper will show by its various deflections all the changes in the illumination of the selenium.

In order to compare these variations we must first adopt a scale: if we suppose the selenium in complete darkness, its resistance will be a maximum, and the deflection of the galvanometer the least. The greatest effect would be to annul the resistance of the selenium; by withdrawing the latter from the circuit we should obtain a greater deflection, which we call 100 or the maximum light. Dividing the interval thus obtained into 100 equal parts, we shall have actinometrical degrees which are always comparable.

In practice the battery to be used would be that of Clamond. During the determination of the 100 point and of zero the external part of the pile would be kept at zero. By repeating the operation at different external temperatures, we should obtain a table for reducing the actinometrical degree obtained at a given temperature to what it should be if the external part of the pile were at zero.—*Comptes Rendus*, Feb. 2, 1885.

ON THE SYNTHESIS OF TRIMETHYLAMINE AND PYRROL FROM COAL-GAS; AND ON THE OCCLUSION OF HYDROGEN BY ZINC DUST. BY GREVILLE WILLIAMS, F.R.S.

In a paper on "The Action of some Heated Substances on the Organic Sulphides in Coal-Gas,"* I mention that on passing coal-

* See 'Journal of Gas Lighting,' vol. xli. pp. 913, 960.

gas that had not been purified by lime over palladized pumice at a temperature considerably below a red heat, ammonia and distinct traces of a volatile alkaloid were formed. The palladium was charged with hydrogen by passing the coal-gas over it at about 212° Fahr. At this temperature palladized pumice absorbs hydrogen from the coal-gas; and at a higher temperature gives it off in such a condition as to react upon the vapour of cyanides and produce ammonia and a volatile alkaloid. When the palladium ceased to act—that is to say, when ammonia and the alkaloid no longer continued to be formed—the tube containing the palladized pumice was transferred to a trough of boiling water, and the current of gas was kept up until the metal had become active—that is, charged with hydrogen. The water-bath was then removed, and the tube was heated by a very small gas-flame, when the formation of ammonia and the alkaloid again commenced. The quantity of the latter formed from 250 grains of palladized pumice containing 20 per cent. of palladium was, however, so small that I could not procure enough for analysis.

Having found that zinc dust heated to a low temperature in a current of coal-gas rendered hydrogen active in a similar way to palladium, I determined to ascertain if, by using a much larger quantity than I had of palladized pumice, the synthesis could be effected on a sufficient scale to enable me to analyze the products. For this purpose I used the following simple apparatus:—A globular flask was fitted with a cork carrying two tubes, one of which passed to within $\frac{1}{2}$ inch of the bottom; and, at its lower extremity, was protected by a cage of fine copper gauze, to prevent it from being stopped up by the zinc dust. The flask was then filled with the latter to the commencement of the neck. The gas passed through the zinc dust with the greatest ease.

Experiment 1.—The flask arranged in the manner described was heated with a small rose-burner; and, when the temperature arrived at about 400° Fahr., sulphuretted hydrogen was evolved, accompanied by the same peculiar odour that was observed when using palladized pumice. When a rod moistened with hydrochloric acid was brought near the exit, it fumed strongly. The odour was like that of fish. On holding a piece of fir-wood moistened with hydrochloric acid in the vapour, the red reaction characteristic of pyrrol was distinctly obtained. The vapour was now passed into dilute hydrochloric acid during the night, and in the morning a scarlet ring was formed on the sides of the beaker about $\frac{3}{4}$ inch above the solution. This was probably pyrrol red, produced, in the well-known manner, by decomposition of the pyrrol by the acid.

Experiment 2.—After the preliminary experiment just described, 19 feet of the gas were passed as before; but the products were conveyed into a Geissler's potash-tube containing hydrochloric acid diluted with twice its volume of distilled water. The product was evaporated to dryness; the chloride of ammonium and tarry matters removed; and the hydrochlorate of the base, which was very

deliquescent, was converted into a platinum salt. It weighed 2·7 grains. The smallest trace of this platinum salt gave the fingers an intensely fishy odour. From its great intensity I inferred that the alkaloid was not methylamine, as when working with palladized pumice I, at one time, thought it might be, remembering Dr. Debus's synthesis of that base by the action of platinum black on prussic acid*. The platinum salt produced in this experiment was not analyzed, but reserved for further purification.

Experiment 3.—The current was kept up for twelve hours, during which 21·51 feet of gas passed. At this point the flask cracked. On cooling it was broken up; and it was found that the zinc had partly melted. On treating it with dilute sulphuric acid, sulphuretted hydrogen was evolved. The platinum salt weighed 4·7 grains. It yielded on ignition 38·83 per cent. of platinum. The corresponding salt of methylamine requires 41·61 per cent.; that of dimethylamine requires 39·53; and that of trimethylamine 37·20. It is not unlikely, however, that traces of methylamine or dimethylamine, or both, were present. It should here be noticed that the errors tend in the direction of too much platinum, owing to the difficulty of removing the last traces of ammonium salt when working on such minute quantities. It was found that ammonia and traces of the alkaloid were formed in this experiment when the zinc dust was cold; but this was not always the case. At 202° Fahr., the fumes with hydrochloric acid were more obvious; at 226° they were observed the instant the rod dipped in the acid was brought near the exit tube. At 242° the fumes were very briskly evolved; and no apparent increase was noticed up to 356°.

Experiment 4.—The gas was passed for 17 hours, to hydrogenate the zinc; and 7 hours with heat. The chloride of ammonium produced weighed 15 grains; the platinum salt amounted to 7·7 grains. On ignition it gave 38·40 per cent. of platinum, or nearly the same as in Experiment 3. The gas passed amounted to 100 feet.

Experiment 5.—Here 101 feet of gas gave 2·03 grains of platinum salt, containing 37·04 per cent. of platinum. This only differs by 0·16 per cent. from the number required by theory for trimethylamine. The sal ammoniac formed amounted to 6·2 grains.

Experiment 6.—The volume of the gas used was not taken. Two precipitates of platinum salt were obtained. The first was put aside to be worked up afterwards with some others to obtain greater purity. The second weighed 7·2 grains and afforded 38·47 per cent. of platinum. The chloride of ammonium weighed 1·9 grain.

Experiment 7.—This time 42 feet of gas were passed. Two crops of crystals of platinum salt were obtained, weighing together 8·0 grains. The mixture yielded 38·05 per cent. of platinum; and 2·1 grains of chloride of ammonium were obtained.

In some of the foregoing experiments the whole of the platinum salts were not used in the determinations of the metal. The

* Journ. Chem. Soc. 1863, p. 249. See also T. Fairley, "On the Action of Hydrogen on Organic Polycyanides," Journ. Chem. Soc. 1864, p. 362.

portions left were therefore mixed, distilled with soda, and the distillate received in hydrochloric acid. The solution was evaporated to dryness, traces of chloride of ammonium were removed, and the solution treated with a very small quantity of platinic chloride. The solution was then allowed to repose until next day; and the platinum salt formed was filtered off and rejected. The filtrate was treated with more platinic chloride dissolved in alcohol. A copious precipitate was obtained. On analysis it gave 14.16 per cent. of carbon, 3.79 of hydrogen, and 37.81 of platinum. Trimethylamine requires 13.57 per cent. of carbon, 3.77 of hydrogen, and 37.20 of platinum. The following is a summary of the above results:—

No. of experiment.	Gas used.	Platinum Salt obtained.	Alkaloid obt. calc. as Trimethylamine. Grains.	NH ₄ Cl obtained. Grains.	Platinum, per cent. in Salt.
1.	—	—	—	—	—
2.	—	2.70	0.6	—	—
3.	21.51	4.70	1.0	—	38.83
4.	100.00	7.70	1.7	15.0	38.40
5.	101.00	2.03	0.5	6.2	37.04
6.	—	7.20	1.6	1.9	38.47
7.	42.00	8.00	1.8	2.1	38.05
Mixture	—	—	—	—	37.81

The mean percentage of platinum is 38.10, which is 0.90 per cent. too high for trimethylamine, and 1.43 per cent. too low for dimethylamine. As the errors of experiment tend to give too high a number for the platinum, I conclude that the base formed was principally trimethylamine. It might also be propylamine, which requires the same numbers, and be formed from cyanide of ethyl as in Mendius's well-known process. Such a supposition, however, does not account for the ammonia formed at the same time— $C_2H_5CN + 2H_2 = C_3H_7N$. But as the ammonia formed simultaneously with the base is in such large excess, further experiments are necessary to explain all the phenomena. The base butylenediamine, obtained by Fairley from cyanide of ethylene, requires 9.59 per cent. of carbon, 2.80 of hydrogen, and 39.45 per cent. of platinum*; and moreover would not have the characteristic smell of trimethylamine.

If we assume that the base is formed from prussic acid, the equation becomes $3CHN + 6H_2 = C_3H_7 + 2NH_3$. The production of sal ammoniac in the experiments was too irregular to enable this equation to be confirmed from its amount.

According to Lange† a polymeride of prussic acid exists having the formula $C_3H_3N_3$. If formed from this substance, the equation is the same as that given above.

It appeared to me that some light might be thrown upon the

* Fairley, *loc. cit.* p. 363.

† *Deut. chem. Ges. Ber.* vi. 99.

question, if hydrogen instead of coal-gas were passed through hydrocyanic acid and then into heated zinc dust; because, if in that case I obtained a number for the platinum in the salt nearer to methylamine, it would show that the formation of trimethylamine from coal-gas did not arise from a peculiarity in the action of the zinc, but from the substance in the gas which gave the alkaloid not being ordinary hydrocyanic acid.

Experiment 8.—Hydrogen prepared from zinc and dilute sulphuric acid was passed through a warm acidulated solution of cyanide of potassium for one day, after a previous passage during the night to render the zinc active. The resulting alkaloid was converted into platinum salt, and weighed 3.20 grains. It gave on ignition 42.59 per cent. of platinum. The platinum salt of methylamine requires 41.68. Error +0.91. The chloride of ammonium amounted to 24.1 grains. It is by no means easy to completely separate so small a quantity of methylamine from so much ammonia; and the excess of platinum is probably due to this cause. The 24.1 grains of chloride of ammonium contained 7.6 grains of ammonia; whereas the 3.20 grains of platinum salt of methylamine only contained 0.41 grain of the alkaloid, or less than half a grain. The equation $\text{CHN} + 2\text{H}_2 = \text{CH}_3\text{N}$ does not involve the formation of any ammonia. If we suppose the base to be formed from cyanogen, we have of course $\text{C}_2\text{N}_2 + 5\text{H}_2 = 2\text{CH}_3\text{N}$, which also dispenses with the formation of ammonia.

The smallness of the quantity of alkaloid formed in the reaction (only amounting to 7.2 grains in the six experiments in which it was estimated) would have prevented me from carrying out this preliminary investigation, if I had not, fortunately, had much previous experience in working on minute portions of these substances. The substance or substances which yield ammonia and pyrrol are not entirely removed by treating the gas with lime; as I found in one experiment that, on passing well-purified gas over heated zinc dust rendered active, turmeric paper was reddened and pyrrol evolved.

The source of the pyrrol I hope to clear up by future experiments; but I may mention that I found, in 1855, that it is formed in numerous cases where nitrogenous animal and vegetable matters are subjected to destructive distillation*.

I must not omit to state that, having on one occasion treated zinc dust with very dilute sulphuric acid, with a view to its purification, the washed and dried product refused to act upon the gas in the manner described in the foregoing experiments. I propose to return to this question.—*Journal of Gas Lighting*, Jan. 6, 1885.

The Gaslight and Coke Company, Nine Elms,
December 1884.

* *Trans. Roy. Soc. Edin.* xxi. part 2.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

APRIL 1885.

XXIX. *Contributions to the Theory of Magnetism.*
By WERNER SIEMENS*.

SINCE the efficient construction of electromagnetic machines has acquired great practical importance, the question has arisen in very various forms, how to choose the form and mass of the magnets so as to obtain the greatest efficiency with the smallest expenditure of material and space. The ingenious theories which have been proposed as the result of the greatest skill and mathematical knowledge seldom give the required indications for the solution of these problems. The reason of this is, no doubt, that the production and distribution of magnetism in magnetic bodies, of which practically only iron in its various molecular conditions is in question, the action at a distance of the magnetism present, and the consequent strength of the magnetic field, and, lastly, the reciprocal action of the latter upon the strength of the magnetism produced in the iron and its distribution, have, as a rule, been separately considered and submitted to calculation. Even if we obtain thus the fundamental data for the solution of the numerous problems proposed, yet the practical man is bewildered by a number of laws and empirical formulæ, which make it impossible for him to obtain a clear conception of the causal connection of phenomena which may serve him as a guide in his designs. This unsatisfactory condition may result from the fact that all magnetic theories started from permanent magnetism, in the same way as electric theories

* Translated from Wiedemann's *Annalen*, vol. xxiv. No. 1, p. 93 (1885).
Phil. Mag. S. 5. Vol. 19. No. 119. April 1885. S

have been based upon the first-known electrical phenomena. But permanent magnetism is only a secondary phenomenon. It is what remains of a previous more intense magnetization whose laws can only be deduced from those of electromagnetism, since magnetism is undoubtedly to be conceived only as an electrical phenomenon. The electric current, or, more generally, electricity in motion, is the only known source of all magnetism. I have already expressed the opinion that this must also hold good of the earth's magnetism, and have assigned as a reason that, so far at least, no other cause is imaginable than the rotation of the electricity accumulated at rest upon the earth's surface with the earth about its axis. The loadstone, and other bodies occurring in nature in a magnetic condition, obviously derive their magnetism from that of the earth, or in particular cases possibly from the direct action of electric discharges.

If, on the other hand, we start with the assumption of a body directly or indirectly magnetizable by means of electric currents, and which retains no magnetism so soon as the cause of magnetism ceases to act, and assume with Faraday that, moreover, the magnetic action, both in the magnetic body itself as well as in the surrounding space, is propagated only from molecule to molecule, or from space-element to space-element, then the further assumption lies close to our hand that both actions—the internal and external—must be completely dependent on each other. In an iron rod round which an electric current circulates there can be, then, only so much magnetism produced by the electric disturbance which acts upon it as is associated in the space surrounding the iron rod with the magnetic disturbance—advancing in the direction of Faraday's lines of force from the north magnetic to the south magnetic superficial elements of the rod—and thus arranged in the form of a magnetic closed circuit.

If this conception is shown by experiment to be allowable, then the laws of the molecular communication of heat, electricity, and electrostatic distribution must also hold good, with the necessary modification for magnetism. We should then be able to lay down a universal law for the intensity of magnetism of the form "Sum of the magnetizing forces divided by the sum of the opposed resistances," which would avoid many difficulties and apparent contradictions. The following further law must also hold good, that "In each sectional plane which cuts all the lines of force the sum of the magnetic moments of all the magnetic molecules cut through = 0." Such a sectional plane can pass only through the magnetic centre of the magnetized body cutting through

neutral south and north magnetism, and then the sum of the magnetic moments of the molecules of iron cut through must be equal in magnitude to that of the molecules or space-elements cut through outside the iron.

The order of the electrical phenomena would then be, that an electrical difference of potential appearing between two bodies situated in an insulating medium would produce upon their surfaces an accumulation of electricities at rest of opposite polarity, the magnitude of which would depend upon the resistance opposed by the surrounding insulating material to the electric disturbance. This resistance depends upon the geometrical conditions, and upon a special coefficient of distribution belonging to the particular material. If the surrounding space is not insulating, but more or less a conductor of electricity, then electric currents are produced the strength of which is again dependent upon the sum of the resistances opposed to the propagation of the electricity. The electric current, or the electricity in motion, has, again, the property of attracting similarly-directed currents, or the bodies conveying them, and of repelling oppositely-directed currents. If we assume, with Ampère, that the magnetic material is filled with preexisting molecular currents, then the electric current must tend to turn these elementary solenoids out of their position of equilibrium, so that their axes fall upon the periphery of circles which concentrically surround the current-carrier. If any particular material (*e. g.* iron) contains a larger number of such circular currents in the unit volume, then the work of the current must be greater, since a greater number of solenoids in which the current produces rotation is contained in each cross-section of the concentric ring. But since, moreover, the strengthening effect which the consecutive cross-sections must produce upon each other, on account of the diminished distance apart of the elementary solenoids, is now greater, therefore, for both reasons, the sum of the moments of a concentric ring consisting of iron must be greater than that of a space-ring of like dimensions, filled with a less magnetic material. This may also be expressed by saying that iron and the other so-called magnetic substances oppose a less resistance to magnetic polarization than the non-magnetic substances, or that their magnetic conductivity is greater. Magnetic action at a distance cannot take place with rings of homogeneous material which encircle a conductor concentrically, since all the lines of force run within the ring. The conditions are different in an iron ring which is not complete. Since the resistance offered by iron to magnetic disturbance is only about $\frac{1}{500}$ of that of air, as appears

from experiments to be described later on, the total magnetism in an interrupted ring must be less, corresponding to the great increase in resistance to the magnetic disturbance caused by the air-filled space of the gap; and the disturbance-lines, or lines of force which bind together the parts of the ring, must occupy the whole surrounding space in very different intensities, and produce in it the phenomena of magnetic attraction and distribution, or those of the so-called free magnetism.

Hence the Amperian theory must be extended by supposing that not only magnetic substances, but all bodies as well as empty space are filled with circular currents of very small dimensions, and that magnetic substances differ only from nonmagnetic substances inasmuch as the number of circular currents present in the unit volume is much greater in the first case than in the second.

All magnetic phenomena may then be referred to the property of the electric current of exerting a directive force upon the molecular solenoids which fill all space, but which are present in greater numbers in the so-called magnetic bodies, which place their axes at right angles to its direction and tend to bring them into closed concentric attraction-circles. The magnitude of this rotation of the axes depends, on the one hand, on the magnitude of the directive or magnetizing force, and, on the other hand, on the number of the molecular circuits preexisting in the unit-volume, for which condition the term "magnetic conductivity" may be employed, or that of "magnetic resistance" for its reciprocal value.

To test the admissibility of this view, the case of an iron ring, or closed ring of iron tube, uniformly wound with insulated wire, which case has been repeatedly examined experimentally and theoretically, seemed to me especially suitable, since, according to G. Kirchhoff's investigations, such a ring produces no magnetic effect at a distance.

For the magnetism of an iron tube having a thickness of wall s , traversed by an axial current, I have previously* obtained the value

$$M = 4\pi lsi$$

from Ampère's formula—where s is the thickness of metal, l the length of the tube, and i the strength of the current—and have shown its correctness by means of experiments.

If we surround an iron ring of sectional area q and mean radius ρ with a closely wound spiral, then, in accordance with the above considerations, the magnetizing force is proportional

* *Sitzungsber. d. Berl. Akad.* 1881, p. 701.

to the current-strength i multiplied by the number of windings, for which we may put the length of the ring $2\rho\pi$ nearly.

The resistance which opposes itself to this magnetizing force is directly proportional to the length of the bent iron rod (that is, again, $2\pi\rho$), and inversely proportional to the cross-section and to the magnetic conductivity of the iron, which may be denoted by ψ . Consequently the magnetic moment of the iron ring in each of its cross-sections is

$$\frac{i2\rho\pi}{\left(\frac{2\rho\pi}{q\psi}\right)} = iq\psi \text{ const.},$$

which expression denotes the same as the previous one,

$$M = ils \text{ const.}$$

To investigate the crucial question whether the magnetism which is produced by a magnetizing force in an iron bar or in an open horseshoe is also inversely proportional to the total resistance of the magnetic circuit, I had a horseshoe made out of iron bar 20 millim. thick, and bent at right angles. The legs of the horseshoe were 70 millim. long, and each was surrounded by a spiral 35 millim. long, consisting of from 126 to 130 windings of insulated wire 1 millim. thick. The straight back of the horseshoe was provided with an induction-coil of 1160 turns of wire, 0.2 millim. thick. A prismatic piece of iron, of the same cross-section as the horseshoe, could be used to make it into a closed metallic circuit. The legs of the magnet projected 20 millim. beyond the coils.

The experiments were made by momentarily reversing the direction of the current through the magnetizing coils by means of a suitable commutator. The strength of the current before each reversal was measured by determining the difference in potential between the terminal screws of the magnetizing spiral by means of a torsion-galvanometer with many coils of thin wire. By interposition of resistances or by using shunts the desired current-strength was easily obtained. The current which the reversal produced in the induction-coil was led through the coils of a nearly aperiodic reflecting-galvanometer. The deflection then measured the double magnetic moment produced in the magnet by the acting current. By taking the precaution to reverse the current several times after each change of current-strength before making the measurement, concordant results were obtained even with the greatest differences of current-strength.

TABLE I.

	1.		2.		3.		4.	
	Leg 90 millim. high.				Leg 70 millim. high.			
	Closed by keeper.		Open.		Closed by keeper.		Open.	
Ampere.	Increase for $\frac{1}{100}$ ampere.		Increase for $\frac{1}{100}$ ampere.		Increase for $\frac{1}{100}$ ampere.		Increase for $\frac{1}{100}$ ampere.	
0·01	800	800	195	195	1095	1095	140	140
0·03	3150	1175	650	227	4800	1852	430	145
0·05	6250	1550	1125	237	10500	2850	750	160
0·07	10500	2125	1640	257	20500	5000	1100	175
0·09	15500	2500	2165	262	33000	6200	1410	155
0·1	18350	2850	2400	235	36200	3200	1570	160
0·15	37000	3730	3700	260	2390	164
0·2	4830	226	3100	142
0·25	6000	234	3900	160
0·3	7100	220	4700	160
0·4	9600	250	6200	150
0·5	12250	265	7900	170

The foregoing Table gives in the first column the observed current-strength, in the first column of each succeeding series the corresponding deflection, and in the second the increase of magnetic moment calculated from it for an increase in current-strength of 0·01 ampere, both in the open condition and when closed. The result is, that in the closed horseshoe the magnetism at first increases in more rapid ratio than the current-strength. With the open magnet for feeble currents (0·05 ampere) the magnetism is about one fifth of the magnetism produced by the like magnetizing force in the closed magnet; for the double current-strength (0·1 ampere) about one eighth. The increase of magnetism, on the other hand, is nearly constant with the open magnet, *i. e.* the magnetism was nearly proportional to the current-strength up to the limit which the wire-coils permitted without too great heating.

Next I cut 20 millim. off the projecting poles of the electro-magnet and repeated the experiments. As is seen in the third and fourth columns of the above Table, the magnetism of the closed magnet was much increased by this shortening of the poles; whilst the magnetism of the open magnet decreased in a still greater ratio, so that now for a current of 0·05 ampere the above ratio has decreased to $\frac{1}{14}$ and for 0·1 ampere to $\frac{1}{23}$. This disproportionately large decrease is evidently to be ascribed to the circumstance that not only has

the resistance to the magnetic disturbance of the surrounding space been increased by the shortening of the magnet, but the magnetizing force has also been decreased, since the coils of wire exerted some magnetic force upon the portions cut off. I endeavoured to determine the length for which the magnetism of the open magnet would, with a constant current, become doubled, by lengthening the poles with pieces of iron of the same diameter and 10 millim. in height. This condition, according to the following Table, was exceeded when five pieces had been added to the poles, corresponding to an increase in length of the original magnet by something more than half.

TABLE II.
Current 0.1 ampere.

	Magnetism.	Increase.
Without addition	1950	...
1 piece on each side.....	2430	480
2 pieces on each side	2805	465
3 " " " "	3330	435
4 " " " " "	3750	420
5 " " " " " "	4125	375

We see from the increase in magnetism for each lengthening of the magnet by 10 millim. that there is a considerable falling off in this increase with the number of pieces added. This is, partly at any rate, a consequence of the stronger direct action of the coils upon those of the added pieces which were nearer to the coils, which also explains the too rapid doubling of the magnetism with increased length. Nevertheless these experiments render it in the highest degree probable that the magnetism produced by a magnetizing force in an open magnet is a function of its surface. This was further confirmed by the observation that the magnetism was increased just as much by placing on the magnet-legs pieces of thin-walled iron tube as by using solid cylinders of equal diameter. Closing the tubes by means of iron covers made no perceptible difference if there was no increase in length produced by doing so.

To determine the resistance opposed to the production of magnetism by the nonmagnetic surrounding space, it was necessary to compare the resistance offered to magnetic disturbance by a space filled with air, or empty, with that of iron. This ratio cannot be constant, since the specific

magnetic resistance of iron varies with the strength of its magnetization.

As is known, and as we may also see from the above experiments, the magnetism in a closed electromagnet increases at first more rapidly than the current-strength. The increase of magnetism then soon attains a maximum, and with further increase of current-strength sinks slowly down to a very small amount. In this behaviour of magnetic substances the initial increasing action of the magnetizing force to a maximum is very striking. The position of this maximum is dependent upon the properties of the iron. With soft iron the maximum is sooner attained with equal magnetizing force than with harder iron. It is therefore not improbable that this initial feebler action of the magnetizing force is always only a consequence of the imperfect softness of the iron. In order to examine this more closely I had two similar rings made, the one of the softest iron rod possible, the other of soft steel. The external diameter was 50 millim. and the internal diameter 35 millim.; and they were similarly wound each with two coils, the lower one of 350 turns of wire 0.2 millim. thick, the upper one of 190 turns of wire 0.75 millim. thick. The second coil served for the magnetizing current, the first as induction-coil. The first column of Table III. gives the current-strengths as measured; the second the corresponding magnetic moments of the iron ring as measured by the deflection of the reflecting-galvanometer; the third the increase in magnetism for an increase in current-strength of 0.001 ampere calculated from these numbers; the fourth and fifth columns give the same values for the steel ring.

TABLE III.

Current-strength.	Iron. Deflec- tion.	Iron. Increase for $\frac{1}{1000}$ ampere.	Steel. Deflec- tion.	Steel. Increase for $\frac{1}{1000}$ ampere.	Current-strength	Iron. Deflec- tion.	Iron. Increase for $\frac{1}{1000}$ ampere.	Steel. Deflec- tion.	Steel. Increase for $\frac{1}{1000}$ ampere.
0.001	3.2	3.2	3.5	3.5	0.100	1810	65	430	5.0
0.002	7	3.25	7.2	3.7	0.15	4520	54	760	6.6
0.004	15	4	15	3.4	0.2	6880	47.2	1120	7.2
0.008	36	5.2	29	3.5	0.25	8640	35.2	1640	10.4
0.01	46	5	35	3.0	0.3	9900	25.2	2500	17.2
0.02	114	6.8	72	3.7	0.4	11500	16	4950	24.5
0.03	196	8.1	112	4.0	0.5	12400	9	7000	30.5
0.04	300	10.5	155	4.3	0.6	13150	7.5	8750	17.5
0.05	410	11	195	4.0	0.7	13750	6	10000	12.5
0.06	550	14	245	5.0	0.8	14250	5	11000	10.0
0.07	710	16	290	4.5	0.9	14600	3.5	11900	9.0
0.08	895	18.5	340	5.0	1.00	15000	4	12550	6.5
0.085	1015	24	360	4.0	1.1	15250	2.5	13150	6.0
0.090	1160	29	380	4.0	1.2	15500	2.5	13600	4.5
0.095	405	5.0	1.5	16150	2.1	15000	4.6

It results from these experiments that the maximum increase of magnetism in the case of the iron ring was attained for a current of 0.1 ampere, whilst with the ring of soft steel the maximum was not reached until the current rose to 0.5 ampere ; and, further, that this latter maximum amounted to only one half of the maximum increase in the case of the iron ring. Since the direction of the current is reversed in each measurement, the residual magnetism could exert no direct influence upon the results of the measurement ; but the internal friction, which opposed itself to the rotation of the hypothetical circular currents, must no doubt produce a decrease in magnetization which would be smaller in the ring of soft steel than the iron ring. It hence appears very probable that, with absolutely soft iron, the maximum action would be attained even with the feeblest currents. We may therefore regard this anomaly which appears in the magnetization of iron as a consequence of the frictional resistance, which hinders the rotation of the Amperian circular currents. This resistance will be so much the more perceptible the smaller the angles of rotation ; since the work done by friction must be proportional to the angle of rotation itself, and not to the magnetic moment produced by the rotation.

The following Table gives the results obtained when, instead of a closed ring, a straight iron bar of equal length and thickness was subjected to magnetization by means of a current of increasing strength. The iron bar was provided in the middle with an induction-coil, and, together with this, was pushed into the middle of a magnetizing-coil of nearly double length. In the deflections of the mirror-galvanometer the direct action of the coils upon each other is allowed for.

TABLE IV.

Current-strength.	Deflection.	Increase for 1000 ampere.	Current-strength.	Deflection.	Increase for 1000 ampere.
0.001	12	12	0.08	1160	16
0.002	22	10	0.09	1320	16
0.004	44	11	0.10	1480	16
0.008	88	11	0.20	2900	14.2
0.01	109	10.5	0.30	4600	17
0.02	233	12.4	0.40	6240	16.4
0.03	365	13.2	0.50	8000	17.6
0.04	524	15.9	0.60	9720	17.2
0.05	688	16.4	0.70	11560	18.4
0.06	844	15.6	0.80	13200	16.4
0.07	1000	15.6	1.00	16800	18

The increases in magnetism, calculated for 0·001 ampere, show here also a small increase with increase of current-strength. The maximum increase could not be attained without unduly heating the coil.

If the assumption is correct, that nonmagnetic material, in the same manner as magnetic material, is filled with pre-existing molecular circular currents, it is to be assumed that in it, as in magnetic material, there exists a maximum of magnetism. An approach to a maximum of magnetization must then become perceptible with very great magnetic moments of a magnetic field, as in iron.

In order to investigate this, I placed two prismatic pieces of iron on the poles of the horseshoe-magnet described, which could be made to approach each other closely. The parallel surfaces opposed were reduced to one square centimetre. The increase in magnetic moment of the magnetic circuit was measured for increasing current-strength. The results are given in the following Table :—

TABLE V.

Intensity of current.	Horseshoe closed by iron plate.		Poles fitted with prismatic pieces of iron. Their distances apart :—					
	Deflection.	Increase per $\frac{1}{100}$ ampere.	Deflection.	0·1 mm. Increase per $\frac{1}{100}$ ampere.	Deflection.	1 mm. Increase per $\frac{1}{100}$ ampere.	Deflection.	3·5 mm. Increase per $\frac{1}{100}$ ampere.
	1.		2.		3.		4.	
0·11	219	19·9	80	7·2	49	4·4	37	3·4
0·21	720	50·1	193	11·3	107	5·8	78	4·1
0·53	1708	30·9	620	13·3	313	6·4	217	4·3
0·85	2124	13·0	1020	12·5	524	6·6	362	4·5
1·06	2292	8·0	1276	12·2	680	7·4	460	4·7
2·12	2640	3·3	2028	7·1	1344	6·3	942	4·5
3·18	2760	1·1	2400	3·5	1908	5·3	1380	4·1
4·24	2840	0·7	2620	2·1	2340	4·1	1820	4·2
5·30	2870	0·5	2728	1·0	2576	2·2	2172	3·3
6·36	2930	0·4	2800	0·7	2700	1·2	2440	2·5

The pole-pieces were placed at distances of 0·1, 1, and 3·5 millim. apart. The current-strength was increased to the highest possible value, *i. e.* 6 amperes. As seen from column 1, with a closed magnet the maximum increase occurred with a current of only 0·2 ampere. It is remarkable that with approximated pole-pieces the turning-point for each distance

apart fell nearly at the same current-strength of about 1 ampere. The increases themselves cannot be in proportion to the distances apart of the parallel faces, since the side faces of the pole-pieces must take so much the more part in the magnetic action the further the parallel faces were separated, and consequently so much the greater must be the magnetic resistance of the interval. That a maximum increase takes place with all distances apart, and that with increasing current-strength this uniformly decreases to a small amount, is a consequence of the large moment of the electromagnet itself, which, in consequence of the small magnetic resistance of the entire circuit, with strong currents approaches its maximum of magnetization. The experiments certainly seem to indicate an increase in the resistance and magnetic disturbance of the nonmagnetic material for very high magnetic moments, but not decisively. For such experiments we must employ electromagnets of small length and so large section that the resistance of the iron to magnetic disturbance may remain very small in comparison with that of the surrounding medium.

The experiments show, however, that a maximum of magnetization does not occur sooner with atmospheric air than with iron. It follows from this that the strength of a magnetic field is limited only by the maximum of magnetism in the iron, and that the magnetic conductivity of nonmagnetic material may be taken as constant. Since this value for iron is very variable and the law is not yet known of its dependence upon the magnetic moment of the mass of iron, there is no possibility of comparing the magnetic conductivity of the two in general. Since, however, in the construction of magnetic machines it is always precisely that magnetic condition of the iron for which the increase of magnetism with increasing current-strength is a maximum which is of the most importance, we must make this point the starting-point of the inquiry.

To investigate this point I had two square iron plates made, 4 millim. thick and 80 millim. square, which could be screwed on sideways to the prismatic movable pole-pieces of the previously described horseshoe-magnets. At a distance of 5 millim. from each other, with a current-strength of 0.1 ampere the plates gave the same increase of magnetism as a piece of sheet-iron of 1 square millim. section which, after screwing off the plates, was made to connect the pole-pieces through a distance of 5 millim.

With feeble magnetizing-forces the iron was the more powerful, with stronger currents the plates. This shows a magnetic conductivity for iron in its condition of greatest

magnetic susceptibility of 480 to 500, when that of air is taken equal to 1. The experiment was repeated with greater distances apart of the plates, and further with iron wire, sheet-iron, and prismatic rods of iron, and the same ratio was obtained.

The question next presented itself whether this resistance of the air, amounting to some 500 times the resistance of iron, was not in part to be ascribed to the magnetic oxygen contained in the air. In order to investigate this, I connected two round iron plates of 8 millim. diameter by means of a ring of brass soldered to them. By means of tubes closed by taps fitting into two openings in the brass ring, it was possible to fill the space between the metal plates, which were 5 millim. apart, with any desired gas or to exhaust the chamber. The iron plates thus connected were fastened to the pole-pieces of the electromagnet, and the magnetic moment of the magnetic circuit measured for different current-strengths. Not the smallest difference could be observed whether the space between the plates was filled with air, oxygen, or hydrogen, or whether it was exhausted as completely as possible by means of the mercury-pump.

It follows therefore that the magnetic properties of oxygen, and in general the influence of matter, with the exception of iron and the other so-called magnetic metals, upon the magnetization only become of influence with very great magnetic moments, such as those with which diamagnetic phenomena occur, and that for nonmagnetic substances, only the geometrical conditions of space need to be taken into account in magnetic phenomena. Whether this will not lead us, in accordance with the views of Secchi and Edlund, to replace the Amperian molecular circuits by ether-vortices which fill all space, but are present in magnetic substances only in greater number or of greater intensity, may remain here undiscussed. The striking fact that a vacuum permits magnetic distribution and attraction like nonmagnetic matter would, however, be explained by it.

That space filled with nonmagnetic material as well as a vacuum is affected by electric currents quantitatively exactly in the same way as iron in its condition of greatest magnetic susceptibility, but some 500 times less energetically, is seen from the following experiment. I had two coils made of insulated wire 1 millim. thick, of 87 millim. diameter, and 100 millim. length, and placed them with axes parallel and at a distance apart of 131 millim. The pole-surfaces of these solenoids opposite each other were each covered by an iron plate, which was wound between the solenoids, with an

induction-coil. The iron plates were held fast together by thin brass bolts which traversed the axes of the solenoids. The two solenoids and the two iron plates formed together a closed horseshoe-magnet, whose magnetic moment was to be measured by means of the induction-coil on the iron plates. In the following Table VI. these measures are brought together. It will be seen from the table that all the phenomena were exactly the same as if the iron plates had been connected by iron cylinders instead of brass bolts. On actually making the experiment with iron cylinders of 4 millim. thickness (which had therefore $\frac{1}{500}$ of the section of the solenoid*), the magnetic moment was, in fact, nearly twice as great as before with a current-strength 0.2 ampere, as shown in the sixth column of the table, which gives the quotients of the deflections with and without iron cylinders. That with feeble currents the quotients are not greatly different from unity, but then rise rapidly to the double value, and then slowly decrease, is due to the property possessed by the keepers of this air-magnet, and the iron cylinder of opposing a great resistance to magnetization, both with very feeble magnetic moments and with very powerful ones.

TABLE VI.

Intensity.	Solenoid alone.		As open horseshoe.		As closed horseshoe. A.		Closed and with iron bolts. B.		Ratio of deflections A and B.
	Defl.	Incr.	Defl.	Incr.	Defl.	Incr.	Defl.	Incr.	
0.01			51	51	55	55	65	65	1.18
0.05			270	55	295	61	380	79	1.27
0.10	52	5.2	580	62	640	68	1000	124	1.56
0.15			900	64	1020	76	1920	184	1.88
0.20			1236	67	1392	74	2864	189	2.06
0.30			1928	78	2160	77	4480	162	2.07
0.40			2616	69	2960	80	5850	137	1.97
0.50	258	5.2	3360	76	3800	84	7200	135	1.89
0.75			5250	77	6075	91	10250	122	1.68
1.00			7240	78	8400	93	12880	105	1.53

As a direct proof of the correctness of the assumption that in an iron bar round which electric currents circulate only so much magnetism can be produced as is connected with the sum of the magnetic moments of the air or space in contact with its surface, the following considerations may be men-

* If we take the diameter of the active air-cylinder as extending to the middle of the coils, then the ratio of sectional areas $\frac{\text{iron}}{\text{air}} = \frac{1}{560}$.

tioned :—If an iron cylinder of radius r , regarded as of infinite length, is, anywhere sufficiently far from the ends, encircled by a coil of wire, and if we denote by y the magnetic moment which a current flowing through this coil gives to the unit section at any desired distance x from the centre of the coil, then the magnetic moment of this section is $r^2\pi y$. This magnetic moment must become smaller as x increases, and, if our theory is correct, by so much as is connected with the moment of the layer of air in contact with it, or the increment dx . Hence the differential equation

$$-r^2\pi dy = 2r\pi dx y,$$

$$-\frac{dy}{y} = \frac{2}{r} dx,$$

$$-\log y \left| \frac{y_1}{y} = \frac{2}{r} x \right|_{x=c},$$

holds good if c denote the interval for which integration is to be performed, or

$$\log \frac{y}{y_1} = \frac{2}{r} c,$$

and for equal displacement, with rods of different diameters $2r$ and 2ρ ,

$$\log \frac{y}{y_1} : \log \frac{z}{z_1} = \rho : r.$$

These equations show, first, that for the same iron cylinder the ratio of the magnetic moments of two sections equally distant from each other is constant upon the whole half-cylinder ; that therefore also equal displacements of a test coil must always result in equal percentage decrease in magnetic moment. They show, further, that with rods of different thickness, and for equal displacement of the test-coil, the logarithms of the ratio of moments vary inversely as the diameter of the rods. In writing the differential equation, however, the assumption is made that the moment of the layer of air in contact with the surface of the rod depends only on the moment of the unit section of the rod at the point in question. This would mean that the resistance to magnetic disturbance of all external magnetic circuits was the same. But in fact the disturbing action takes place between each element of the surface of one half of the rod and all the oppositely magnetic points of the other half. It is therefore also dependent on the distance from the centre of the rod. This source of error will become so much the more important the less the distance from

the middle of the rod to the point at which the displacement through the distance c takes place.

TABLE VII.

Distance from the centre of the rod. millim.	ϕ 9 millim.		ϕ 6 millim.		ϕ 3 millim.	
	Deflec- tion y .	$\frac{y}{y_1}$.	Deflec- tion y .	$\frac{y}{y_1}$.	Deflec- tion y .	$\frac{y}{y_1}$.
90	4268	0.032	4054	0.057	290	0.112
100	3960	0.037	3558	0.052	222	0.116
110	3640	0.035	3160	0.052	168	0.121
120	3360	0.035	2800	0.059	130	0.111
130	3100	0.038	2440	0.053	100	0.114
140	2840	0.032	2160	0.056	80	0.097
150	2640	0.038	1900	0.048	60	0.125
160	2420	0.037	1700	0.058		
170	2220	0.028	1488	0.052		
180	2080	0.039	1320	0.056		
190	1900	0.033	1160	0.058		
200	1760	0.040	1016	0.060		
210	1605	0.040	884	0.059		
220	1465	0.032	772	0.052		
230	1360	0.033	684	0.051		
240	1260	0.036	608	0.061		
Mean value.....	0.035		0.055	0.114
Mean value \times thickness of rod	} 0.105		0.110	0.114

The above experiments confirm the assumption that there is no such thing as free magnetism, but only bound magnetism, and that a magnetic force can produce in magnetic bodies only so much magnetism as is bound in them and in the surrounding medium by magnetic distribution in the form of closed attraction-curves with equal magnetic moment in each section. This mode of regarding the matter is exactly analogous to the theory of electric molecular distribution; and hence the laws which hold good for this may be applied to magnetic distribution; and, by the aid of the coefficient 480, which expresses the ratio of the resistance of air to that of iron, the influence of the mass and form of iron to be employed in the construction of a magnetic field may be determined.

If an iron bar, upon the middle of which a magnetizing force acts, may not be regarded as endless, the formula calculated for the endless bar,

$$\log \frac{y}{y_1} = \frac{2}{r} x,$$

s not directly applicable on account of the magnetism of the end-surfaces.

The distribution of magnetism in a bar of definite length is quite different if the magnetizing force acts equally on all parts of the bar. The decrease in magnetic moment from the middle of the bar towards the ends loses then its logarithmic character, and assumes the form of a catenary (as already shown by Rees), or nearly that of a parabola. In a rod 150 millim. long and 7.70 millim. diameter, which was enclosed in a glass tube of double length uniformly wrapped with a magnetizing-coil, the magnetic moment of each section could be determined by means of an induction-coil wound over the middle of the tube, when the direction of the magnetizing-current was reversed.

In the following Table the magnetic moments are given for the same rod at distances of from 20 to 70 millim. from the centre when the bar was uniformly magnetized, and when magnetized at the centre. The current-strengths were so chosen that the magnetism at the end of the rod was nearly the same in both cases.

Distance x of the secondary coil from the centre of the rod.	Deflection when magnetized uni- formly.	Deflection when magnetized at the middle.
millim.	millim.	millim.
20	287	463
30	263	378
40	233	302
50	195	229
60	145	160
70	87	92

Calculating from the equation to the parabola

$$\frac{x^2}{y} = 2p,$$

the deflections of the second column for uniform magnetization give for $2p$ the values 23, 22, 22, 23, 23, 22. Calculating from the numbers of the third column by means of the formula

$$\log \frac{y}{y_1} = \frac{2}{r}x,$$

the quotients $\frac{y}{y_1}$ for a constant displacement of the induction-

coil of 10 millim., we obtain the numbers 1.25, 1.25, 1.32, 1.4, 1.4. The ratio of the magnetic moments of points of the rod equidistant from each other is therefore not constant, as in the case of the endless rod, but increases as we approach the end of the rod, as indeed was to be expected.

Van Rees found that in a homogeneous prismatic magnet the magnetic moments of the sections decrease parabolically, as with the uniformly magnetized iron bar. This, however, holds good only for those magnetic bars to which, in the process of magnetization, a uniform moment has been communicated through the entire length. As soon as the magnetizing force has ceased to act, all the molecular magnets have the same tendency to return again to the unmagnetized condition, whence at last the same condition of magnetic equilibrium results, as with the uniformly magnetized iron bar.

Lastly, I should like to make a few further remarks on a previous communication of mine*.

I proposed the theory at that time, that the molecular magnets assumed in the Ampère-Weber theory must consist each of two elementary magnets or solenoids close together with opposed poles, capable of free rotation together in any direction without meeting with resistance, but directed by external magnetizing forces, and separately rotated, as would be the case with astatic pairs of needles capable of free and independent rotation. I did not then know that Stephan had already expressed the same view, and accompanied it by important mathematical considerations. Now, in accordance with the theory explained above, the Amperian theory must be extended, as already mentioned, by supposing all space to be filled with paired molecular solenoids, or, if we adopt Edlund's view that the electric current consists in æther in forward motion, filled with æther vortices, and that these are present in magnetic material in greater numbers than in nonmagnetic material. Since, now, a magnetizing force acting upon the molecular magnets only exerts a perceptible influence on the rotation apart from each other of the paired elementary magnets when all the neighbours in the magnetic circuit follow the motion, and so are able to produce a closed system of equilibrium capable of mutual attraction, it follows that the rotation directly produced by the magnetizing force must be very small in comparison with the mutual strengthening of the rotation in the closed magnetic circuit. The magnetic moment produced must thus be essentially the product of the mutual strengthening of rotation of which the magnetizing force is the cause. Here the difficulty is presented that the rotation

* *Loc. cit.* p. 703.

ceases again, in the absence of coercitive force, as soon as the magnetizing force ceases to act. Such a condition of equilibrium cannot be imagined as produced otherwise than by the simultaneous action of attractive and repulsive forces. There must then be produced a nearly, but not quite, unstable equilibrium of the elementary magnets by the mutual action of all the neighbouring attractive and repulsive molecular forces, if the assumptions of Ampère's theory are to correspond to facts. That a combination of molecular forces fulfilling this necessary requirement can be shown to be possible I do not venture to assert.

XXX. *On the Seat of the Electromotive Forces in the Voltaic Cell.* By Professor OLIVER J. LODGE, D.Sc.

[Continued from p. 190.*]

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7. **T**HE result of the survey in regard to our special subject of discussion may be summed up thus:—(1) that there is certainly an E.M.F. at the junction of two different substances, or even of the same substance in two different states; and (2) that the total E.M.F. of a circuit is the algebraic sum of all such contact-forces at every junction in the circuit. I do not know that these two propositions could be passed *nem. con.*, but I believe that, provided they are properly understood, the dissenting minority would be a very small one. It is probable that Professor Exner would be in the minority, but I am unable to be sure of any one else.

We can also make a negative proposition which will command almost universal assent—viz. that if in the above second proposition, instead of the sum of the contact-forces at *every* junction, we attend only to the contact-forces at the *metallic* junctions, the proposition will no longer be true. This fact, that the metallic junctions are insufficient to account for all the E.M.F., was established by Becquerel, De la Rive, and

* Scarcely any of this portion was read at Montreal, but it was given in substance to the Society of Telegraph Engineers on March 26th.

others, and still more thoroughly and exhaustively by Faraday. It is the easiest possible thing to make a number of batteries which shall give a current without any metallic junction whatever. Faraday gives some thirty of them*.

One more certain proposition we can lay down—viz. that whenever a current is produced, the energy of the current must be maintained by absorption of heat, or by chemical action, or by gravity, or by some other such agent, not by mere contact.

So much being agreed to, what remains as subject-matter for controversy? This: A voltaic circuit contains at least three junctions; what is the value of the contact force at each of them? and especially to which junction is the major part of the observed E.M.F. due? Is it the zinc-acid? or is it the copper-acid? or is it the zinc-copper? There is no other question. The old chemical and contact controversy has died out, but another controversy remains. Most physicists probably would say today that the major part of the E.M.F. of the cell resides at the zinc-copper junction. This was Volta's view, and this is the view of the text-book writers taught by Sir William Thomson. Some few would say at the zinc-acid junction, and among them I must confess myself.

It is no question between contact and something else; it is a question between a feeble energy-less metal-metal contact, and an active energetic metal-fluid contact with potentialities of chemical action straining across the junction. What is there to distinguish between the two? Electrostatic experiments with air-condensers prove nothing. They add up three E.M.F.s, $\text{air}/M + M/M' + M'/\text{air}$, and give you the sum. The experimenters usually assume that M/M' is what they are measuring, but there is no proof to be given in support of the assumption, except that if you substitute water for air the effect remains almost unaltered; but then water contains oxygen as the active element the same as air does. Well, then, it may be urged, the effect is the same *in vacuo* and in hydrogen as in air; and to this I answer, Not proven.

Can any further assertions be made with reference to electroscopic experiments as bearing on voltaic theory? Yes, it can be asserted that by adding up the Volta effects for A/B ,

* Exp. Res. ii. 2020. Dr. J. A. Fleming describes another of these batteries in Phil. Mag. June 1874, and gives some very cogent and readable arguments in favour of the "chemical theory" of battery E.M.F., suggesting that the difference of potential between the terminals of a battery on open circuit is due to potential chemical combination of the metals and electrolytes. He does not, however, explain the old Volta experiment; and, as Prof. Chrystal has pointed out (*Encyc. Brit.*, "Electricity," p. 99), upholders of the chemical theory are bound to explain this.

for B/C, for C/D. . . and for Z/A, you arrive at the total E.M.F. of the circuit A, B, C A. True, but what then?

The Volta effect you call A/B is really

$$\text{air/A} + \text{A/B} + \text{B/air} ;$$

that you call B/C is

$$\text{air/B} + \text{B/C} + \text{C/air} ;$$

.

and that you call Z/A is

$$\text{air/Z} + \text{Z/A} + \text{A/air}.$$

Add them up, and you get

$$\text{A/B} + \text{B/C} + . . . + \text{Z/A},$$

which *must* be the E.M.F. of a circuit by common sense—*i. e.* without violent experimental disproof, which no one has ever attempted to give. This fact, that the sum of the Volta effects equals the sum of the true forces, in a closed circuit of any conducting materials, has nevertheless caused persons to suppose that air/metal forces are negligibly small. But it is clear that they may have any value they like without affecting the truth of the law. They could only affect it if air/M were not equal to $-M/\text{air}$. The experimental proof of the summation law, therefore, establishes that air/M is equal to $-M/\text{air}$, as well as the important fact that the contact-force at each junction is independent of all other junctions of what kind soever.

8. Leaving electrostatic determinations as without bearing on the point at issue, let us ask, Is there no direct and straightforward way of measuring the actual E.M.F. at a particular junction without disturbance from other junctions? The answer is most clearly given by Clerk Maxwell*, thus:—

“ Sir W. Thomson has shown that if Π is the coefficient of Peltier effect or the heat absorbed at the junction by unit current in unit time, then ΠI is the E.M.F. at that junction acting with the current. This is of great importance, as it is the only method of measuring a local E.M.F.; the ordinary method of connecting up by wires to an electrometer being useless. This Peltier measurement is quite independent of the effect of contact-forces in other parts of the circuit. But the E.M.F. so measured does not account for Volta’s force, which is far greater and often opposite. Hence the assumption that the potential of a metal is to be measured by that of the air in contact with it must be erroneous, and the greater part of Volta’s E.M.F. must be sought for, not at the junction of the two metals, but at one or both of the surfaces

* ‘Electricity and Magnetism,’ vol. i. art. 249. Abbreviated here, because so easy of reference.

which separate the metals from the air or other medium which forms the third element in the circuit."

And in another place he says* :—

"In a voltaic circuit the sum of the E.M.F.'s from zinc to electrolyte, from electrolyte to copper, and from copper to zinc, is not zero, but is what is called the E.M.F. of the circuit—a measurable quantity. Of these three E.M.F.'s only one can be measured by a legitimate process, that namely from copper to zinc. If we cause an electric current to pass from copper to zinc, the heat generated in the conductor per unit of electricity is a measure of the work done by the current, for no chemical or other change is effected. Part of this heat arises from the work done in overcoming ordinary resistance within the copper and the zinc. This part may be diminished indefinitely by letting the electricity pass very slowly. The remainder of the heat arises from the work done in overcoming the E.M.F. from the zinc to the copper, and the amount of this heat per unit of electricity is a measure of the E.M.F. Now it is found by thermoelectric experiments that this E.M.F. is exceedingly small at ordinary temperature, being less than a microvolt, and that it is from zinc to copper†. Hence the statement, deduced from experiments in which air is the third medium, that the E.M.F. from copper to zinc is .75 volt, cannot be correct. In fact, what is really measured is the difference between the potential in air near the surface of copper, and the potential in air near the surface of zinc, the zinc and copper being in contact. The number .75 is therefore the E.M.F., in volts, of the circuit copper, zinc, air, copper, and is the sum of three E.M.F.'s, only one of which has yet been measured."

With every word of Maxwell I cordially agree.

9. While on the theoretical aspect of the subject, it may be well to see what Pellat, as one of the best experimenters on it, has to say. Pellat substantially observes as follows :—

"Does the apparent difference of potential between two metals in contact indicate a real difference of potential between them? In all rigour, No! but the slight variation of its value when different gases or even liquids are used renders it extremely probable that there is such a real difference of potential, and that it is very nearly what is measured in electroscopic experiments."

As to difficulties connected with energy considerations

* Maxwell: Letter to the 'Electrician,' April 26, 1879. Also 'Elem. Electricity,' p. 149.

† Further on (sect. 23) I point out that this statement is not quite true, but it does not affect the main argument.

and mere contact, he refers to Helmholtz* and Clausius†, who, he says, relieve him of all responsibility on this head.

The fact that the voltaic order of the metals is much the same as their order of oxidizability must have struck nearly everybody, and must also have been felt as a difficulty by the upholders of the efficacy of mere contact. Pellat considers he disposes of it thus :—"Since the E.M.F. of a pile is that represented by chemical action, and since by experiment voltaic contact-forces have much the same values as the E.M.F. of piles, it follows that there is some vague relation between A/B and the heats of combination, say of substitution of one metal for another in a salt (as in a Daniell)."

He sums up his experimental conclusions as follows :—

(1) "Two different metals united metallico are covered, in the state of equilibrium, with electric coats of unequal potential."

(2) "This difference of potential only depends on the superficial coat of metal. It changes notably when the surface is mechanically scratched, becoming always more positive. As the scratching effect disappears with time, so does the extra difference of potential. The state of polish of the surface is immaterial, but traces of foreign substances, forming a coat so thin as to be invisible, are able to modify the value of the observed effect enormously."

(3) "The effect depends somewhat on temperature."

(4) "The pressure and nature of the gas surrounding the metals have a very distinct but extremely feeble influence, but, since the effect produced is a lagging one, it is probably due to some secondary cause, and it is probable that the difference of potential is really independent of the gaseous dielectric."

(5) "The difference of potential between the electric coats on two metals united metallico has the same value as the E.M.F. of an element of a liquid pile formed by these two

* *Die Erhaltung der Kraft*, p. 47, where Helmholtz develops Volta's original hypothesis about an attraction of matter for electricity, of an amount depending on the kind of matter, so that it gets pulled one way or another across a junction of two dissimilar substances. He points out that the Volta effect is explained if zinc be granted a stronger attraction for electricity than copper has. This view he returns to in his Faraday Lecture 1881, where also he refers to Berzelius's electrical theory of chemical affinity. The opinions of Professor Helmholtz are too weighty to be merely referred to in a footnote, but we may have occasion to consider them later.

† *Die mechanische Behandlung der Elektrizität*, chap. vii. §§ 2 and 3, where Prof. Clausius follows up the above idea by considering the rôle which heat plays in the matter, and thus hypothetically explains the Peltier effect also.

metals, provided that the E.M.F. is determined before any alteration of the metallic surface wetted by the liquid has occurred; but these alterations produce themselves very rapidly."

Pellat's theoretical conclusions being short may also be here quoted, and I will number them on with the others.

(6) "It is extremely probable that the difference of potential between the electric coats which cover two metals connected metallicity represents the true difference of potential which exists between them. No reason, either theoretical or experimental, can be invoked against the existence of a difference of potential between two metals in contact."

(7) "This last quantity has no connection with the thermoelectric E.M.F. measured by the Peltier phenomenon."

(8) "It has only a vague and distant connection with the difference of oxidizability of the metals."

Concerning these propositions, I may remark that while number 2 is likely to annoy contact theorists (though I know they have methods of explaining it away), numbers 4 and 5 are calculated to restore their equanimity. The five experimental conclusions I accept as in duty bound, only permitting myself to doubt the perfect generality of numbers 4 and 5 under all circumstances; but the three theoretical ones I am unable to wholly accept. Thus with respect to the second part of number 6, I beg entirely to differ from M. Pellat if I am called on to simultaneously admit number 7. Whether one is prepared to accept any of his theoretical conclusions or to reject them all, depends upon how one regards them. If in the way he himself intended, then I reject them all. If with one's own interpretation, then I say that the second part of 6 and 8 are true (though for "only a vague and distant" I would substitute "no"); and 7 is also true if it be held to refer to the quantity first mentioned in number 6, while number 8 refers to the other quantity. Number 6 I should also consider true if the prefix "im" be made to the fourth word.

10. Pellat then proceeds to explain why he considers the Peltier effect to be quite distinct from, and have no relation to, the true E.M.F. of contact. In explaining this he makes use of a piece of unpleasantly plausible reasoning, which I myself have heard Professor Ayrton use, and which, when unexpectedly suggested, is so painfully benumbing that it is worth while to quote it, and to indicate its weak point. Pellat's statement of the argument is rather long; perhaps it can with advantage be abbreviated.

Two metals A and B put into contact are at different potentials, the difference A/B being due to and equal to the E.M.F.

of contact. There is, then, at the junction not only the contact-force E , but also the equal opposite force $-dV$, due to the difference of potential established. Either of these forces alone would resist or aid the passage of electricity across the junction and so give rise to a Peltier effect, but both together will do nothing of the sort; and so if there be any Peltier effect, it must be some small residual phenomenon, or it must be due to some other and totally distinct cause*.

Professor Ayrton's way of putting the argument, which I think he said he got from Sir William Thomson, was something like this. When Q units of electricity are transmitted against a force E , work EQ is done; also when they are transmitted up a difference of potential $V' - V$, work $Q(V' - V)$ is done; but, in an open circuit containing an electromotive junction, $V - V'$ is produced by and is equal to E . Hence, at an electromotive junction no work need be done by a current; in other words, the existence or non-existence of a Peltier effect has nothing to do with the existence or non-existence of a local E.M.F.

The fallacy of the argument, in either form, lies in over-precise specification of locality; it gratuitously asserts as true for *the junction* what is only proved to be true for the whole circuit. It assumes that there can be no work done *at a junction* if it be perfectly easy to drive electricity either way across it—*i. e.* if there be no work done *on the whole*.

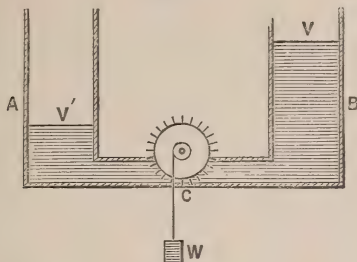
11. To exhibit the fallacy, consider a hydrostatic analogy. Two vessels of water connected by a pipe in which is a motor of some kind, which, without leakage, exerts a specified force on the water and maintains a constant difference of potentials, but then remains stationary, doing no further work. We typify it feebly in the diagram by an impracticable close-fitting water-wheel driven by a weight without friction.

$V - V'$ is the equivalent of the force exerted at the junction, and everything is in equilibrium. It is perfectly easy for water to flow from one vessel into the other under the influence of the slightest extra force, for W helps the water up the hill $V - V'$, when the flow is in that direction; and, whenever the flow is reversed, it lets the water gently down again, taking all its energy out of it. If water is made to flow from A to B , say by pouring more into A , the weight W is lowered, or energy disappears (heat absorbed) at the junction; if it is made to flow from B to A the weight is raised, or energy

* Thus it may be, suggests Pellat, due to a slight difference between E and $-dV$ produced by the mere fact of a current passing; *i. e.* contact E.M.F. with electricity at rest may be slightly different from what it is with electricity in motion.

(say heat) is generated at the junction. Thus there is a true Peltier effect at the junction despite the existence of $V - V'$ and its equality to the junction force, and yet no resistance is offered to the flow of water either way. Thus is the first form of the argument controverted.

Fig. 14.



Hydrostatic analogue of the true contact or Seebeck force, and of the real though small difference of potential which it maintains between two metals in contact. A and B represent the two metals, C their junction; W is a weight driving a water-tight wheel until stopped by the difference of potential set up. The hydraulic raising or lowering of the weight represents the Peltier effect.

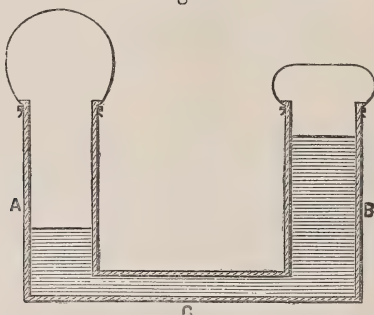
To pump water from A to B by any other pipe would need work to be done equal to $Q(V - V')$, and to pump water against the force of W, acting alone, would also need work EQ ; but when the water goes from A to B *viâ* W, or *vice versâ*, no work is done on the whole. Quite true: but the conclusion that no work is done *at the junction* by no means follows. Work *must* be done at the junction in proportion to the force there (by inspection of the diagram); and accordingly the existence or non-existence of a Peltier effect has *everything* to do with the existence or non-existence of a local E.M.F. This controverts the second form of the argument.

If the argument be now considered upset, are we to proceed to assert that the difference of potential, or force, concerned in the Volta effect, and the heat-destruction or generation concerned in the Peltier effect, are closely connected, and in fact different ways of observing the same thing? By no means. All we have proved is that the Peltier effect accurately and necessarily represents and measures the true contact-force at a junction. True we have considered a difference of potential $V - V'$ as produced by this contact-force in an incomplete circuit; and so it is. But nothing has been said to imply that this difference of potential has anything to do with what is observed in electrostatic experiments as the Volta effect. So far from this, I will assert that what is

usually observed when two metals are touched and separated is not primarily a difference of potential between the metals at all. They are at different potentials when separated, no doubt, because they are oppositely charged; but they may have been at the same potential until separated. The real Volta effect is almost independent of the true contact-force, and of the difference of potential which it produces. In other words, a good Volta effect can be observed when there was no difference of potential whatever between the metals when in contact.

According to my view the Volta effect is produced, not by a contact-force at the junction of the two metals, but by a

Fig. 15.



Hydrostatic analogue of the Volta effect, or *apparent* difference of potential produced by metallic contact, and of the opposite charges but uniform potential which it maintains between two metals in contact. The vessels are covered by air-tight elastic bags differently stretched.

contact-force at their free surfaces, between the metals and the air or other medium surrounding them. To represent this hypothesis by a hydrostatic model, we shall have to maintain the difference of level in the two connected vessels, not by a force at the junction, but by a force at the surfaces; say by using closed vessels and compressed air, or more pictorially by differently stretched elastic membranes or bladders tied over the tops of the vessels.

Note that the difference of level in this case implies no difference of potential; and, as before, no work is required to transfer water between A and B. Hence it is not easy to distinguish this case from the former; and this difficulty of distinguishing between the two cases is what has given rise to most of the confusion. The only easy criterion is the non-existence in the second case of any Peltier effect at the junction C. Naturally it is possible and common for the two effects to be superposed, but they are essentially independent.

Since the two vessels in the second case are at the same potential, the way to observe the effect is to cut and seal the

pipe at C, and then show that the vessels are differently charged; which is what Volta did. The model does not indeed represent the gradual change of potential induced as the distance between the condenser-plates increases; and it is scarcely worth while to complicate the matter by making a more elaborate model. The thickening of the dielectric layer of a condenser, when its plates are separated, corresponds exactly to the thickening and strengthening of an elastic membrane; and rise of potential in the one case is accurately representable by increase of pressure in the other; but such considerations belong to general electrostatics, and have no special bearing on our present subject.

12. This is perhaps the most convenient place to introduce the notes or condensed statements which I drew up and distributed at the meeting before the discussion. They were intended to be critically exact (allowing of course for mistakes and possible slips) so as to bear analysis; and hence it is probably worth while to reproduce them here with notes and comments.

I. Orthodox Statements believed by the writer to be true in the form here set down.

A. Volta.

i. Two metals in contact ordinarily acquire opposite charges*; for instance, clean zinc receives a positive charge by contact with copper, of such a magnitude as would be otherwise produced under the same circumstances by an E.M.F. of about .8 volt.

ii. This apparent contact E.M.F. or "Volta force" is independent of all other *metallic* contacts wheresoever arranged; hence the metals can be arranged in a numerical series such that the "contact-force" of any two is equal to the difference of the numbers attached to them, whether the contact be direct or through intermediate metals. But whether this series changes when the atmosphere, or medium surrounding the metal, changes, is an open question: on the one side are experiments of De la Rive, Brown, Schulze-Berge; on the other side, of Pfaff, Pellat, Thomson, Von Zahn †. It certainly changes when the free metallic surfaces are in the slightest degree oxidized or otherwise dirty. And in general this "Volta force" is very dependent on all non-metallic contacts.

iii. In a closed chain of any substances whatever, the

* Observe that it is not said that two metals in contact acquire different potentials. Such difference of potential I believe to be only apparent. Compare figs. 14 and 15.

† I put Von Zahn on that side because he himself considers himself there, and because the great bulk of his experiments lean decidedly that way.

resultant E.M.F. is the algebraic sum of the Volta forces measured electrostatically in air for every junction in the chain : neglecting magnetic or impressed E.M.F. [Verified most completely by Ayrton and Perry.]

B. Thomson.

iv. The E.M.F. in any closed circuit is equal to the energy conferred on unit electricity as it flows round it.

[Neglect magnetic or impressed E.M.F. in what follows.]

v. At the junction of two metals any energy conferred on, or withdrawn from, the current, must be in the form of heat. At the junction of any substance with an electrolyte, energy may be conveyed to or from the current at the expense of chemical action as well as of heat.

vi. In a circuit of uniform temperature : if metallic, the sum of the E.M.F.'s is zero by the second law of thermodynamics ; if partly electrolytic, the sum of the E.M.F.'s is equal to the sum of the energies of chemical action going on per unit current per second.

vii. In any closed conducting circuit the total intrinsic E.M.F. is equal to the dynamical value of the sum of the chemical actions going on per unit of electricity conveyed ($\Sigma J\theta\epsilon$), diminished by the energy expended in algebraically generating reversible heat.

viii. The locality of any E.M.F. may be detected, and its amount measured, by observing the reversible heat or other form of energy there produced or absorbed per unit current per second. [This is held by Maxwell but possibly not by Thomson *, though its establishment is due to him.]

II. *Statements believed by the writer to be false though orthodox.*

ix. Two metals in air or water or dilute acid, but not in contact, are practically at the same potential†. [Sir William Thomson, Clifton, Pellat.]

* The only reason which I can think of as likely to have caused Sir William to doubt or deny the validity of this proposition is given and, I hope, refuted at sections 10 and 11.

† The truth or falsity of this statement may be held to depend on a question of words, viz. the definition of potential. Sir William at the meeting said he had always defined potential as the work done in bringing a unit charge close up to, but not *into*, the body. This definition explains some apparent inconsistency in one or two of his utterances which I had never quite understood. But seeing that there is no difficulty whatever in giving a charge up to a metal body, but rather the contrary, why not define its potential in the more simple manner which followers of his have unconsciously, and I believe universally, adopted, not knowing that they were thus putting themselves out of harmony with him ? Given his definition, so that the potential of a body means really not its potential

x. Two metals in contact are at seriously different potentials (*i. e.* differences of potential greater than such millivolts as are concerned in thermoelectricity). [This is held by nearly everybody*.]

xi. The contact-force between a metal and a dielectric, or between a metal and an electrolyte such as water and dilute acid, is small †. [Ayrton and Perry, Clifton, Pellat, and probably Sir William Thomson.]

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13. Before proceeding to the statements embodying my own views, it will be more interesting if I try to explain in a fuller and more connected manner what they are ‡.

Let us regard the air as a dielectric bath of oxygen, in which metals are immersed, and picture a piece of zinc surrounded by oxygen molecules which are straining at it, and

but the potential of the medium close to it, statements Nos. ix. and x. are undoubtedly true; and No. xi. is also true, I suppose, for it then only means that there is not much E.M.F. between the medium close to a metal and that at a little distance.

* It is much more natural to suppose that the potential of a metallic conductor is uniform, whether it is homogeneous or not. Indeed, it is not only more natural, but it is true, that two parts of a conductor *can* only differ in potential by reason of an E.M.F. located at the junction. Now there usually is an E.M.F. at a junction, but it is only of such a magnitude as is concerned in thermoelectricity. This, indeed, does produce a difference of potential between the metals, but nothing else can. N.B. Always provided that by "the potential of a metal" is meant that potential, and not the potential of air near it.

† The experiments supposed to establish this, really prove only that there is very little difference between the air and the water in which a metal is partially immersed. I do not quite know how to understand, on Sir William's plan, the potential of a metal which is half in one medium and half in another.

‡ The reason I set them forth at length is because I had no time at the meeting both to open the discussion and also to properly express my own ideas, and Sir William Thomson was kind enough to tell me to write out the paper completely, and to explain the position I took up fully. This, therefore, I have endeavoured to do.

endeavouring to combine with it. They may indeed partially succeed ; but suppose they do not, we have here a strong potential chemical action or chemical strain, which must probably be accompanied by some physical phenomenon. Now remember that oxygen is an electro-negative element ; and without endeavouring to examine too precisely what signification is involved in that statement, it will be not out of accord with orthodox views if we assume that it means that at least any dissociated oxygen atoms are negatively charged, each with the characteristic charge of a free dyad atom. Granting something equivalent to this, without pressing the form of expression too closely, we perceive that the strain of the oxygen towards the zinc will result in what I metaphorically call a slackening up, or attempted compression, of the negative electricity in it; *i. e.* in a rise of negative potential. We may therefore say that zinc is at a lower potential than the air surrounding it, and that the step of potential in crossing the boundary from zinc to air is closely connected with the chemical affinity between zinc and oxygen. Observe that this step of potential does not obviously nor probably depend on the amount of oxygen present. It is possible that a few million molecules may be as effective as a large number. Note also that the step of potential is by no means caused by actual oxidation : in so far as the zinc surface is tarnished by oxidation the strain will be diminished and the step of potential become less.

Nothing is said here about the possible effect of the nitrogen, because it is simplest in the first instance to ignore it, though whether experiment will justify this simplicity or not, I do not yet know.

We may go further and assert that if in general the chemical affinity of two substances can be measured by their energies of combination, then the step of potential in the present case may perhaps be calculable from the heat of combustion of zinc.

And one may justify this assertion thus. Let an atom of oxygen combine with an atom of zinc ; it will generate an amount of heat h , and its characteristic charge, q , will be given up to the zinc and will thereby fall down the step of potential, v , which separates the zinc from the air. *Now if we suppose that the heat h is the representative and equivalent of the fall of energy $q v$, it follows of course that*

$$v = \frac{J h}{q}.$$

Make the hypothesis and see what comes of it.

14. The oxidation energy of zinc per gramme-equivalent (*i. e.* 65 grammes of zinc or 16 of oxygen) is, according to the determinations of Julius Thomsen, Andrews, and Favre and Silbermann, 85,430, 84,825, and 83,915 respectively.

The amount of electricity needed to deposit a gramme-equivalent of zinc, or of any dyad element, is, according to the modern determination of Lord Rayleigh *, 19,320 units.

Hence the value of $\frac{h}{q}$, which is a ratio evidently independent of the number of atoms dealt with, lies between $\frac{85400}{19320}$ and $\frac{84000}{19320}$ probably. Let us say it is $\frac{85000}{19320}$ or 4.4.

Now J in absolute measure is 42×10^6 ; so the value of v , which is $\left(\frac{Jh}{q}\right)$ according to the above hypothesis, comes out 1.85×10^8 , that is 1.85 volt.

This then, I say, is the step of potential between zinc and air. (To avoid circumlocution I will henceforward speak as if the above hypothesis were admittedly true, and all I now say stands or falls with it.)

All clean bright zinc is thus about 1.8 volt below the potential of the air near it; tarnished or oxidized zinc will exhibit less difference; and it is perhaps possible that perfectly oxidized zinc need show no difference of potential at all between itself and the air. The step of potential by no means therefore depends upon the occurrence of oxidation; it is the oxidation *tendency* which causes it, but so far as oxidation actually takes place the step diminishes.

Proceed to consider a piece of copper similarly. Oxygen molecules are straining at it also, but with less force. The combustion energy of copper per gramme-equivalent is given by the three authorities already quoted as 37,160, 38,290, and 43,770 respectively. These do not agree well, and it is difficult to know which to take; but Thomsen's results are, I believe, generally relied on; so, assuming his, the step of potential between copper and air will be $\frac{.42 \times 37200}{19320}$ volts, that is about .8 volt. This then is the amount by which clean bright copper differs from the air. Oxidized copper will differ less. Comparing this value for copper with that just obtained for zinc, we perceive that a piece of zinc and a piece of copper are, when separate, not at the same potential; they differ by about a volt from each other. If the zinc is oxidized, they differ less: if the copper is oxidized, they differ more.

* "4.025 grammes of silver are deposited by an Ampère current in an hour" (Montreal Address). This gives the electrochemical equivalent of silver .01118, and of hydrogen .00010352.

Now put the zinc and copper into direct metallic contact, and neglect for the present the third of a millivolt of E.M.F. developed at the junction, which acts so as to drive positive electricity from copper to zinc. A rush of electricity must take place from the copper to the zinc to equalize their potential; it is impossible that they can remain at different potentials when directly united: all parts of a conductor must be at a uniform potential, and the rush has taken place because they were not so when put into contact.

15. Picturing to ourselves the effect as produced by the straining oxygen atoms, we shall perceive that they could not get at either metal when separate: first, because they surrounded it everywhere, and strained equally on all sides; and second, because, being all charged with negative electricity, they could not move in on all sides at once without, so to speak, compressing the electricity in the body and giving it an absolute charge. But directly the copper touched the zinc the oxygen atoms were cleared away at the point of contact, and the stress of those at the rest of its surface was no longer counterbalanced. Moreover, they *can* now all move nearer to the zinc because a way of escape for electricity is provided into the copper, whose surrounding oxygen atoms will be thus driven back somewhat further from the surface, until the dielectric strain, assisting the chemical strain on the copper surface and opposing it on the zinc surface, prevents further displacement, and equilibrium is again attained. The electricity which escaped from the zinc to the copper was negative electricity (oxygen being essentially an electro-negative element), the negatively charged oxygen atoms have moved a little nearer to the zinc than their normal distance: *i. e.* the thickness of its layer of negative electricity is reduced, or its surface is positively charged; the negative layer on the copper has been slightly thickened—its surface is negatively charged.

This is a pictorial way of representing the process, and may be regarded as somewhat fanciful; it is, however, the way in which the theory originally occurred to me, and it permits more insight into the processes than a mere statement in terms of potential can; though it may well be that the imagined processes are but distant likenesses of the real ones.

The oxygen atoms have moved nearer to the zinc; it is now more easily oxidized than before. The copper, on the other hand, is by contact with zinc somewhat protected.

Observe that the contact has not developed any force; it has only, by sweeping away the oxygen from the point of contact, enabled previously existing forces to do work and produce their effect.

The air surrounding the metals in contact is in a state of slight dielectric strain, such as would be produced by two pieces of any one metal of similar size and position, charged so as to differ in potential from each other by a volt.

Zinc and copper plates in contact may therefore be regarded as the plates of a condenser, but they form a peculiar condenser, for they are not really at different potentials; the whole step of potential which throws the air into its state of dielectric strain is located on their bounding surfaces.

Let s and s' be the electrostatic capacity of the zinc and the copper respectively; think of them as two independent spheres united by a fine wire; and let x be their common potential; then the zinc has, by the fact of contact, gone up $1.8 + x$, and the copper has gone down $x - .8$; and, since the quantity of electricity which left the one went to the other, it follows that—

$$s(1.8 - x) = s'(x - .8)$$

$$\text{or} \quad x = \frac{1.8s + .8s'}{s + s'}.$$

There is no necessary relation between s and s' in general, but in the ordinary form of the Volta experiment the two plates are of equal size and shape; in which case $s = s'$, and $x = 1.3$ volt below the potential of the unconstrained air.

All this is wholly unlike a condenser investigation. To treat it as a condenser we must consider the air-surfaces close to the two metals as the plates of the condenser, and we can then speak of its electrostatic capacity S in the ordinary way, and say that it is in the present case charged with the E.M.F. of a volt.

The quantity of electricity on either plate of such a condenser is $S(1.8 - .8)$ or S ; hence

$$s(1.8 - x) = s'(x - .8) = S(1.8 - .8).$$

And the general relation between the three S 's is like that of two Leyden jars in cascade, viz.,

$$S = \frac{s s'}{s + s'}$$

If the two pieces of metal are circular disks each of radius r , and at a distance z apart, the value of S is of course $\frac{r^2}{4z}$.

In Volta's form of the experiment, two plates are put near each other, connected for an instant, then removed from each other, and the potential of one of them observed. This

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measures $\frac{S(1.8 - .8)}{s + x}$, where x is the capacity of the electrometer used.

In Pellat's form the charges acquired by contact are neutralized, and the oxygen atoms driven back to their normal distance, by an extraneous E.M.F. carefully adjusted until the plates, after separation, exhibit no charge. This measures $1.8 - .8$ directly.

16. So far I have spoken as if I were sure that (granting the hypothesis) the potential of clean zinc is 1.8 volt below the air; but I am not really sure that this is anything better than an approximation. The fact that no actual combination occurs makes the matter perhaps a little indefinite. If an oxygen atom unites with a zinc atom, one has a right to say distinctly that q has stepped down v ; but suppose they are only facing one another, and wishing to combine, are we justified then in asserting that the step v is ready for q to go down, and that it is the same v as before? It almost seems to depend on whether chemical attraction becomes greater as two atoms approach one another, or becomes less.

Suppose, first, it becomes greater, which is the natural hypothesis, then the v calculated from data obtained by permitting the combination to occur will be too large for the step of potential caused by the attraction of metal for oxygen over a standard distance. On the other hand, the differential force urging electricity across a junction of two metals, which is observed in the Volta effect, may be somewhat greater than simply the differences in their pull reckoned at standard distances, because the approach of atoms to the zinc will increase it on this side, and the recession of atoms from the copper will decrease it on that. Hence the Volta effect may perhaps be expected to agree better with calculation than the air/metal potential-difference does, if this latter could be experimentally observed, which it never yet has been.

Next, suppose that chemical attraction becomes less as atoms approach: the step of potential between a metal and air will now be greater than that calculated from chemical data; nevertheless the Volta effect will be somewhat less than that due to the differences of such steps for two metals, and may thus possibly agree pretty well with calculation.

The agreement or non-agreement of Volta effects with calculation does not therefore quite establish the accuracy of our calculated metal-air contact-forces. But we have no right to assume that even Volta effects will agree with calculation particularly well so long as our data are so slender. They have no chance of accurately agreeing unless the metals used

are pure and perfectly clean—a most difficult condition to attain for even a few seconds.

Before leaving this subject it may be well to point out that, whereas the *calculation* of a Volta effect depends on data obtained by allowing oxygen atoms to approach the metal so as completely and actually to combine, the *experimental determination* by Kohlrausch's and similar methods depends on letting the atoms approach somewhat nearer to one metal and recede somewhat further from another; while the *compensation form* of the experiment employed by Pellat and others depends upon forcing back and restoring the atoms to their original or standard positions. Now if the views here just expressed have any sense or signification whatever in actual fact, it would be very natural to suppose that the numbers obtained in these three ways might be slightly different. But to specify the direction in which we should expect the differences, if any, to lie would require us to have made up our minds as to the probable variation of chemical attraction with distance. Assuming an inverse variation, the Pellat method should give the least, the Volta or Kohlrausch method the next, and the calculation method the greatest, value for the Volta effect.

But all these ideas complicate the matter somewhat, and they are quite possibly unnecessary. If it be considered that we have no data at present, it may be permitted to work on the simplest hypothesis, viz. that the step v is independent of how nearly chemical action has occurred—that it is the same for atoms straining at one another at their normal distance as for atoms on the verge of combination. And it may be argued in favour of this view that we really have some data, viz. these.

If it were not true, results obtained by Pellat's method could not be expected to agree exactly with those obtained by Kohlrausch's (of which Ayrton and Perry's or Clifton's may be taken as the best examples). Now results obtained by these different methods do agree very fairly well; exact agreement cannot be predicated, for the most trifling circumstances cause large variations in the Volta effect, but no decided disagreement is observable. Again, if it were not true, the Volta effect observed when two metals far apart in the series (*e.g.* zinc and platinum) were employed would be inconsistent with the results obtained by using metals nearer together, say zinc and tin, or tin and platinum; and if this were so, the metals could not be arranged in the linear series which eighty years ago Volta showed they could be. These arguments throw no light on what may happen *just* before actual combination, still they are encouraging as far as they go.

17. Let us therefore endeavour to suppress further qualms, and calculate a series of metal-air contact-forces from the heats of combustion; remembering that all we have to do, in order to convert heats of combustion per dyad gramme-equivalent into volts, is to divide by $\frac{19320 \times 10^8}{42 \times 10^6}$, that is by 46,000.

But the decision as to what numbers we shall take to represent heats of combustion is a matter of some difficulty, for not only do the numbers obtained by different observers for the same reaction differ, sometimes considerably, but it is not obvious when different oxides are formed which of them we are to consider as most applicable to the case of the Volta experiment. Perhaps one should take the most common or stable oxide; perhaps, seeing that no combination is supposed actually to occur, and since the metal is, so to speak, in excess, it is most reasonable to take the lowest oxide which the substance will form. It may be that the data are not known for this; it may even be that they have only been obtained for the hydrate instead of for the oxide.

I must therefore do the best I can, and quote several numbers wherever there is obvious doubt. I imagine that J. Thomsen's are the most reliable when they are available.

But it must be remembered all through, that since it is only the *tendency* to chemical action which is the cause of the Volta effect, whereas combination-heats are obtained by permitting or causing the combination to actually occur, the numbers obtained by calculation are not likely to be quite right; and they may be expected to err on the side of excess, the calculated number being higher than the actual value if directly observed.

Energy of Combination of Metals with Oxygen.

Metal.	Molecule.	Authority.	Heat of formation.	The same reduced to Volts.	Extreme values.	Mean or probable value.
Zinc	Zn,O	Thomsen	85430	1.85		
		Joule	77000	1.66		
		Andrews	84825	1.84		
		Favre and Sil-bermann	83915	1.82		
Tin	Zn,O,H ₂ O	Thomsen	82600			
	Sn,O	Andrews	67100	1.46		
	Sn,O,H ₂ O	Thomsen	68090			
	$\frac{1}{2}(\text{Sn},\text{O}_2)$	Andrews	67680			
Lead	SnO,O	"	69584			
	Pb,O	Thomsen	50300	1.15		
	"	F. and S.	55350	1.20		

Table (continued).

Metal.	Molecule.	Authority.	Heat of formation.	The same reduced to Volts.	Ex-treme values.	Mean or probable value.
Iron	Fe_2O	F. and S.	75656	1.64		
	$\text{Fe}_2\text{O}, \text{H}_2\text{O}$	Thomsen	68280	1.48		
	$\frac{1}{3}(\text{Fe}_2\text{O}_3)$	"	63730			
Nickel... ..	$\frac{1}{2}(\text{Fe}_3\text{O}_4)$	Andrews.....	66448	1.44		
Cobalt	$\text{Ni}_2\text{O}, \text{H}_2\text{O}$	Thomsen	60840	1.32		
	$\text{Co}_2\text{O}, \text{H}_2\text{O}$	"	63400	1.38		
Copper	Cu_2O	"	37160	.81		
	"	Andrews.....	38290	.83		
	"	F. and S.	43770	.95		
	Cu_2O	Thomsen	40810	.89		
Mercury.....	Cu_2O	Joule	37486	.81		
	Hg_2O	Thomsen	30660	.67		
Silver	Hg_2O	"	42200	.92		
	Ag_2O	"	5900	.13		
Hydrogen ...	"	F. and S.	12226	.27		
	$\text{H}_2\text{O} \begin{cases} \text{gas} \\ \text{liquid} \end{cases}$	Thomsen	68360	1.49		
	"	"	78000	1.70		
	"	{ Dulong, Hess, } Grassi, Joule, } Berthelot	79600	1.72		
Potassium ...	$2(\text{K}, \text{H}, \text{O})$	Thomsen	208000			
	$2(\text{K}, \text{H}, \text{O}, \text{aq.})$	"	232920			
	$\text{K}_2\text{O}, \text{aq.}$	"	164560	3.56		
	K_2O	Woods	136000	2.95		
Sodium	$2(\text{Na}, \text{H}, \text{O})$	Thomsen	204060			
	$2(\text{Na}, \text{H}, \text{O}, \text{aq.})$	"	223620			
	$\text{Na}_2\text{O}, \text{aq.}$	"	155260	3.36		
	Na_2O	Woods	151600*	3.28		
Calcium	"	Hypothetical.....	130000	2.82		
	Ca_2O	Thomsen	131360	2.85		
	$\text{Ca}_2\text{O}, \text{H}_2\text{O}$	"	146400			
	$\text{Ca}_2\text{O}, \text{aq.}$	"	149400	3.24		
Barium	Ba_2O	"	130380	2.84		
	$\text{Ba}_2\text{O}, \text{H}_2\text{O}$	"	148000			
	$\text{Ba}_2\text{O}, \text{aq.}$	"	158260			
Strontium ...	Sr_2O	"	130980	2.84		
	$\text{Sr}_2\text{O}, \text{H}_2\text{O}$	"	148180			
	$\text{Sr}_2\text{O}, \text{aq.}$	"	157780	3.41		
Magnesium .	Mg_2O	"	145860	3.16		
	$\text{Mg}_2\text{O}, \text{H}_2\text{O}$	"	148960			
Palladium ...	$\text{Pd}_2\text{O}, \text{H}_2\text{O}$	"	22710	.50		
Cadmium ...	$\text{Cd}_2\text{O}, \text{H}_2\text{O}$	"	65680	1.43		
Thallium ...	Tl_2O	"	42240	.92		
Manganese...	$\text{Mn}_2\text{O}, \text{H}_2\text{O}$	"	94770	2.05		
Aluminium .	$\frac{1}{3}(\text{Al}_2\text{O}_3, 3 \text{H}_2\text{O})$	"	129600	2.81		
Lithium	$\text{Li}_2\text{O}, \text{aq.}$	"	166520	3.62		
Arsenic	$\frac{1}{3}(\text{As}_2\text{O}_3)$	"	51530	1.12		
Bismuth.....	$\frac{1}{3}(\text{Bi}_2\text{O}_3)$	Woods	13200	.29		

* This number for sodic oxide agrees with Thomsen's value for the hydrated oxide, whereas for K, Ba, Sr, Ca the oxide is distinctly below the hydrate. By analogy, one would expect to have to subtract some 25,000 from the hydrated oxide $\text{Na}_2\text{O}, \text{aq.}$, and this gives the 130,000 which I put down as a hypothetical number for Na_2O .

Numbers obtained from unsatisfactory oxides and hydrates like those of aluminium, arsenic, and bismuth, are not likely to be useful for our present purpose. I know of no better data yet available however.

It is now easy to write down a Volta series obtained by pure calculation from heats of combustion. We can then see how far it agrees with the results of direct experiment. The principle on which I determine which of the preceding numbers to select is simply to choose Thomsen's when it refers to the simplest oxide, and in other cases to take what one can get. Metals about which there is obvious uncertainty, as for instance sodium, aluminium, bismuth, &c., are omitted.

Calculated Volta Series.

Lithium and Magnesium	3·0	Nickel	1·32
Potassium	2·95	Lead	1·15
Calcium, &c. . . .	2·84	Thallium	·92
Zinc	1·85	Copper	·81
Iron	1·64	Mercury	·67
Tin	1·46	Palladium	·5
Cadmium	1·43	Silver	·13
Cobalt	1·38		

To compare this series with those obtained by experiment, we may as well take zinc as the metal of reference, and write down the Volta effect between it and the other metals, first as abstracted from the above table, and then as found by different observers. Strictly, one ought first to subtract Peltier forces from the observed numbers before comparing them with theory; but these forces are too small to make any appreciable difference.

Volta Effects in Air.

Comparison between Theory and Experiment.

Metal pair.	Calculated from heats of oxidation.	Observed by				
		Pellat.		Ayrton and Perry.		Clifton.
		Clean.	Scratched.	Using commer- cial zinc.	Using amal- gamated zinc.	
Zinc:—						
Tin.....	·39	·25	·35	·28	·46	
Lead	·70 or ·65	·15	·31	·20	·35	
Iron	·21 or ·4	·56	·70	·60	·74	·75
Nickel	·53	·47	·63	
Copper	1·04 or ·9	·71	·86	·75	·89	·85
Mercury ...	1·18	1·06	1·20	1·07
Silver	1·71 or 1·58	·91	1·12	
Platinum	·98	1·13	

The alternative calculated number sometimes given is merely to show the kind of variation probable in those cases from uncertainty of data. In each case of agreement the calculated number is a little higher than the observed, as was to be expected. No reason occurs to me for the breakdown and apparent interchange, in the case of lead and iron, but such vague guesses as may occur to every one.

Measurements of the E.M.F. between clean metals plunged into distilled water or weak acid, have been made by Clifton and by Beetz*. I suppose one is justified in calling them:—

Volta Effects in Water.

Metal pair.	Calculated from heats of oxidation.	Observed by	
		Beetz.	Clifton.
Zinc:—			
Copper	1.0	.98	82 to 92
Silver.....	1.7	1.23	
Platinum	1.8 or less.	1.52	1.3 (Smee).
Sodium amalgam:—			
Zinc	1.0	.78	
Copper	2.0	1.79	
Silver.....	2.6	2.05	
Platinum	2.8	2.31	

I do not wish to blink the fact that some of the numbers in the former of the above tables afford a rather poor support to my theory; but it must be remembered, on the other hand, that they are not relative numbers only that we have calculated, but absolute; and the fact that the heats of combustion reduced to volts are numbers of the same order of magnitude as the Volta effects, is of itself a strong confirmation of the belief that chemical strain at the air-contacts is the real cause of the apparent contact-force at the junction of two metals.

The agreement of the numbers, though not exact, seems to me too close to be the result of accident. One may, I think, claim that the hypothesis whence the calculated numbers are obtained is justified by the figures as far as they go. It is not put forward as a completed theory, but only as a first step to such a theory. I believe it to be a step in the direction of the truth, but it requires working out and elaborating by a scientific chemist.

18. Not many measurements of metal pairs have been made, even in air; for mere permutations such as copper-tin, tin-silver, &c. follow at once from the numbers given above by Volta's series-law; but in gases other than air one has at

* *Ann. der Physik*, x. p. 348 (1880).

present no experimental guidance beyond the barest qualitative one given by Mr. Brown, that copper-nickel reverses its sign when changed from air to hydrochloric acid, and that copper-iron is reversed in sulphuretted hydrogen. But satisfactory observation in these gases is difficult, because they not only *tend* to attack the plates, but they *do* attack them; and so a film is formed and everything is rendered uncertain.

Another complication results from the fact that, when metals are taken out of air and put into a foreign gas, they are already coated with a film of oxygen, and it is not clear in what way this will affect the action of the new gas. It may have to be replaced almost by substitution; the affinity to be considered in chlorine, for instance, being something like $M, Cl_2 - M, O$. In a compound like HCl , the hydrogen also may have to be provided for, the resulting chemical strain being, for instance, $M, Cl_2 - M, Cl + H_2, O - 2(H, Cl)$; but the consideration of the hydrogen affinities will not affect *differences*, and therefore will leave comparisons with experiment unaffected. Taking the metals as clean, however, and without air-films, we must suppose the following series to be right.

Energies of Combination of Metals with Chlorine; and
Calculated Volta Series in that Gas.

Metal.	Molecule.	Authority.	Heat of Combination.	Calculated Volta series, in Volts.
Zinc	Zn, Cl_2	Thomsen.	97210	2.1
	"	F. and S.	100592	
	"	Andrews.	101316	
Lead	Pb, Cl_2	Thomsen.	82770	1.8
	"	F. and S.	89450	
Iron	Fe, Cl_2	Thomsen.	82050	1.78
	"	F. and S.	99302	
Nickel	No data.		
Copper	Cu, Cl_2	Thomsen.	51630	1.12
	"	F. and S.	59048	
	"	Andrews.	60988	
Mercury	Hg, Cl_2	Thomsen.	63160	1.38
Silver.....	$2(Ag, Cl)$	"	58760	1.28
	"	F. and S.	69600	
Hydrogen ...	$2(H, Cl)$	Thomsen.	44000	.96
	$2(H, Cl, aq.)$	"	78640	
	$2(H, Cl)$	F. and S.	47566	
	$2(H, Cl)$	Alvia.	48174	
	$2(K, Cl)$	Thomsen.	211220	
Potassium ...	"	F. and S.	201920	4.62
	"	Andrews.	208952	
Sodium	$2(Na, Cl)$	Thomsen.	195380	4.24

This series will hold, as far as *differences* are concerned, for hydrochloric acid also; because, whatever effect the hydrogen

affinity may have in changing the numbers, it will have the same effect on all.

It is easy to write down the hypothetical series in bromine and iodine in the same way:—

Calculated Volta Series in Bromine and Iodine.

Metal.	In Bromine.	In Iodine.
Potassium	3·85	3·50
Zinc... ..	1·74	...
Lead	1·40	·87
Silver.....	1·10	·60
Copper	·89	·23
Hydrogen	·40	—13

All this supposes the metals to be perfectly clean, and not covered with a film of foreign gas like oxygen. On the hypothesis that the metal has been taken recently out of air, and that the film of oxygen with which it is covered has to be torn from the metal, though it was not actually combined with it, the Volta series in chlorine or hydrochloric acid would be quite different, and more like this.

Hypothetical Volta Series of Air-coated Metals in Chlorine or HCl.

Silver	1·8
Lead	1·2
Iron	1·1
Copper	·9
Zinc	·85

Unfortunately I can get no data for the heat of chlorination of nickel; but assuming it not very different from iron, the above series gives copper and nickel in the right order, as observed by Mr. Brown, whereas the other one did not. I have no experiments with which to compare the numbers.

Calculated Volta Series of Clean Metals in Sulphur or Sulphuretted Hydrogen.

Metal.	Heat of Sulphurization per gramme equivalent.	Volts.
Potassium	91276	1·95
Zinc	41880	·87
Iron	35506	·76
Lead	19112	·41
Copper	18266	·39
Silver.....	11048	·34
Hydrogen	5482	·12

But it will be observed that this is nothing like the order to be expected in sulphuretted hydrogen ; for it is popularly known that copper is more easily sulphurized in this gas than iron. Now assuming that the metal had been covered with an air-film, and that the oxygen of this has to be replaced by sulphur, the chemical tendency, instead of being M, S , is something more like $M, S-M, O$, or possibly $M, S-M, O+H_2, O-H_2, S$; and either of these will give a quite different order. Data are given on p. 624 of Naumann's *Gmelin-Kraut*, vol. i., for the neutralization-heats of various bases by H_2S , such as $CuO+H_2S$, &c. These are something like what we want, and from them we reckon the following :—

Hypothetical Volta Series, in Sulphur or Sulphuretted Hydrogen, of Metallic Oxides, and possibly of Air-coated Metals.

Metal.	Heat of reaction, $MO+H_2S=MS+H_2O$.	Volts.
Silver.....	55800	1·20
Mercury	41700	1·04
Copper	31600	·68
Lead	26600	·57
Zinc	19200	·41
Iron	14600	·31
Sodium	7700	·16

The series so obtained gives copper and iron in their proper order ; but it is scarcely likely to be really correct, because it assumes that the *oxides* of the metals are exposed to the gas rather than the metals themselves. It is quite possible that it is not very incorrect for tarnished metals, *i. e.* metals coated with a film of oxide ; but for ordinarily clean metals, coated, not with a film of oxide, but with a film of oxygen, it is nothing but a rough approximation, given because we have no better data.

It is to be noted that, as the film of oxygen diffuses away, the Volta-effect depending on it must diminish ; until at length the active affinity causing the chemical strain is nothing more than M, S or perhaps $M, S-H_2, S$. A gradual falling off and ultimate even reversal of sign was observed by Mr. Brown in both HCl and H_2S . In so far as actual chemical action occurs and a film of chloride or sulphide forms, so far of course also will the effect diminish ; because it depends essentially on the unsatisfied chemical strain, not on the accomplished chemical action.

Additional Note, March 13th, 1885.

On page 176 of the March Number of the Philosophical Magazine I quote from Von Zahn and others an extraordinary statement, ascribed to Professor Exner, that a thermopile ceases to work in a vacuum; saying at the same time that I had been unable to find the statement myself in Exner's writings. For a sufficient reason as it now turns out, because it is not there.

Turning over some back volumes of 'Nature' recently, I accidentally came across a letter from Prof. Exner in vol. xxiii. p. 170, December 1880, denying that he had ever said such a thing, and maintaining that his memoirs were quite clear on the subject of the relation, or rather want of relation, between voltaic and thermoelectric phenomena. This is perfectly true; Prof. Exner has expressed himself on this subject with decision, and finds fault with Prof. Edlund for ever confusing the two distinct things together.

In the same volume of 'Nature,' on page 312, appears a note from Professor Young acknowledging that he ought to have been more careful to refer to the original statement, and giving one of the "Physical Notes" of 'Nature,' p. 156, vol. xxii., as his authority. This "Physical Note" quotes from *l'Electricité*; and so the error, however originally manufactured, gets handed on from one paper to another.

One of the memoirs which I have unfortunately overlooked in my historical sketch of the experimental work done in connection with the subject has come to hand this morning, through the kindness of the authors, MM. E. Bichat and R. Blondlot, of Nancy. It was published in the *Comptes Rendus* (1883), t. xc. pp. 1202 et 1293, and in the *Journal de Physique*, 2^e série, ii. p. 533, and is an attempt to measure the Volta-effect for two liquids. The method employed involves the use of three vessels, A, B, C, of which A and C contain one liquid and B the other. A and B are then connected by a liquid siphon with porous diaphragm [the capillary E.M.F. possibly excited by this diaphragm is bad]; and the potential of the air between B and C is then rendered uniform [or, commonly speaking, the potentials of B and C are equalized] by making one of the liquids drip inside a cavity kept moist with the other. A and C are connected by platinum electrodes through a compensation resistance-box and battery to a quadrant-electrometer, and the difference of potential between them measured. The arrangement is highly ingenious, but the difficulties of definite and certain measurement must be considerable.

The same physicists have quite recently made a determination, not of the Volta-effect, but of the true contact-force between two liquids, by the use of mercury electrodes; employing for this purpose a theorem of Helmholtz, that if by judicious polarization the capillary constant of a mercury-liquid surface be made a maximum, then between the mercury and the liquid there is no difference of potential*.

Their account of this last determination has not yet been published, but it will appear shortly (before this is printed probably) in the *Comptes Rendus*.

The theorem gives evidently an admirably simple method of measuring liquid / liquid contact-force, if it can be made to work practically.

MM. Bichat and Blondlot apparently hope, by a comparison of the two methods, the results of which are quite different (in accordance with the views expressed in the present communication), to be able to obtain values for air / liquid contacts. This would be a most important and useful piece of work: the only difficulty in accomplishing it is the difficulty of getting both sets of measurements thoroughly dependable. I hope they may succeed in overcoming all obstacles.

[To be continued.]

XXXI. *The Uses of a Line-Divider.*

By Miss SARAH MARKS†.

THE ordinary method of dividing a given straight line AB into any number of equal parts (fig. 1), without using a special instrument, is to draw a line AC at any angle to it, to cut off from AC the given number of equal parts AD', D'E', &c., to join M, the last of the points so taken, to B, and from D', E', &c. to draw lines parallel to MB, meeting AB in D, E, &c. These points D, E, &c. divide AB into the required number of equal parts. This is exactly the method adopted in using the line-divider.

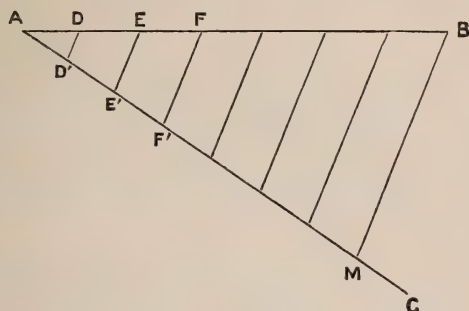
It consists of a hinged rule with a firm joint, having the left-hand limb fitted to slide in an undercut groove upon the plain rule. Both limbs are bevelled on their inner edges, and the left one is divided both on the bevel and on the top into eighths, quarters, half-inches, and inches, which are consecutively numbered, beginning at the hinged end, so that any

* *Monatsberichte der Berliner Akad.* November 3, 1881.

† Communicated by the Physical Society. Read February 14, 1885.

set may be used. The plain rule has two needle-points on its underside to prevent it from slipping when placed in any

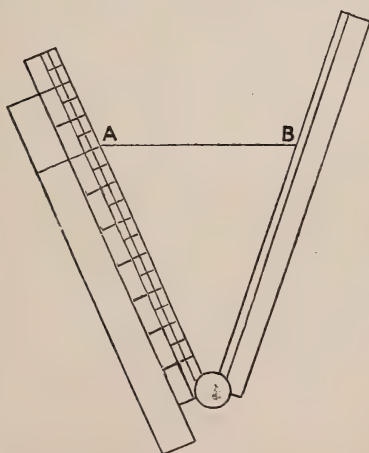
Fig. 1.



position, and on the top has a single line drawn perpendicular to its longer edges.

To divide the line AB into any number of equal parts, say seven (fig. 2): slide the plain rule along the divided limb till its

Fig. 2.



line coincides with one of the lines of this limb against which the number 7 is marked (it is generally best to use the largest possible divisions), place the corresponding line on the bevel

of the limb on the point A, and open or close the rule till the bevel of the undivided limb is on the point B. Now press the plain rule down so that the needle-points enter the paper, and keep it in position. Slide the hinged rule up till the line numbered 6 on the line of reference is coincident with the line on the plain rule, and mark the point where the bevel of the undivided limb meets the line AB. Continue to move the hinged rule up one division at a time till the whole line is divided. It is evident that the ordinary method has been used; for the divided limb forms a line at some angle to AB, and is divided into seven equal parts, and against the undivided limb I can draw parallel lines through the points of section of the divided limb cutting AB in points equidistant from one another.

This is the first method of using the instrument, and is useful in many ways:—to artists, for squaring out their canvases when they wish to enlarge or diminish their drawings, although the second method would perhaps be best for this if a very large divider were used; to decorators, who frequently find it necessary to divide lines into given numbers of equal parts in order to get patterns into a certain space; it would also be useful for finding the divisions on the scales of thermometers, as the distance between the freezing- and boiling-points is always an unknown length.

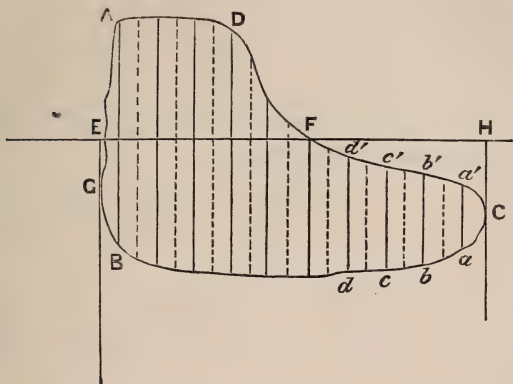
The second method of using the instrument is this:—Let ABCD be an area to be divided into any number of parts, say five, by equidistant lines parallel to AD or BC. Produce AD both ways: place the bevelled edge of the undivided limb along BC, open or close the rule till the end of one of the lines marked 5 on the bevel of the divided limb is on the line AD. Slide the plain rule up, taking care that it is held high enough for its points not to tear the paper, till its line coincides with the 5-line, and press the points firmly into the paper. Now slide the hinged rule up till the division marked 4 coincides with the line on the plain rule, and draw a line along the undivided limb cutting AB and CD. Bring each of the other three divisions against the line on the plain rule, and draw lines as before. These are the lines required.

It is evident that, since the angle ABC may be any angle, it may be a right angle, so that lines can be drawn perpendicular to AB, dividing it into any given number of equal parts. This method is used in finding the mean pressure in gas- and steam-engines by means of an indicator-diagram.

Let A B C D (fig. 3) be an indicator-diagram, E F the atmospheric line; G E, H C, tangents at the extremities of the diagram perpendicular to E F. To find the length of the line

which represents the mean pressure of the engine, divide the area E H C G into ten parts by lines parallel to H C and E B,

Fig. 3.



and equidistant from one another; bisect each of the areas thus obtained by lines parallel to the last. Let the points where these last lines meet the diagram be a' , b' , &c. Add together the lengths a' , b' , &c., and divide by 10; the result will give the line representing the mean pressure of the engine. This process of dividing the diagram, first by ten equidistant lines and then bisecting the spaces between these lines, is generally a very troublesome one, effected by means of a system of parallel rulers and a T-square. In the figure the dotted lines are those not required when the line-divider is used.

To divide the diagram by means of the rule, place the rule as if for dividing the area by 20 equidistant lines parallel to H C. Draw the first of these lines, and then every alternate one, and the diagram is divided as required.

This method of dividing areas is also convenient for drawing treads and rises of stairs, joists, roof-timbers.

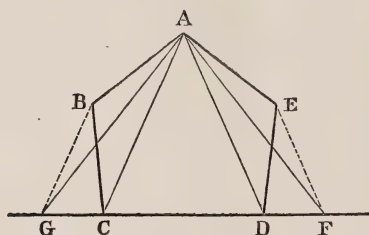
In the third method the divider is used simply as a parallel ruler. As such it has the advantage of having a wide range, and of being firmly fixed at the same time. In graphical statics, where it is often necessary to draw a line parallel to another at a considerable distance from it, set squares have often to be placed and replaced five or six times before the necessary line can be drawn. This instrument will at once draw the line. The range as a parallel ruler may be still further increased by having the plain rule considerably longer than the hinged one.

The special advantages of this instrument as a parallel ruler may best be seen by drawing what surveyors call the "give and take line."

When two fields are divided by a curved boundary, it is often required to find a straight boundary which will divide the fields in the same proportions as before. This straight boundary is what is called the "give and take" line.

In order to understand the method of finding it, it will be necessary to recall the method of finding a rectangle which shall be equal to a given polygon.

Fig. 4.



Let $A B C D E$ (fig. 4) be the given polygon: produce $C D$ both ways, and join $A D$ and $A C$.

Draw $E F \parallel$ to $A D$, and $B G \parallel$ to $A C$, and join $A G$, $A F$. Then $\triangle A F G$ is equal in area to the polygon $A B C D E$. For $\therefore \triangle$'s $A F D$ and $A E D$ are on the same base $A D$ and between the same \parallel 's $A D$, $E F$,

\therefore they are equal.

Similarly $\triangle A G C = \triangle A B C$.

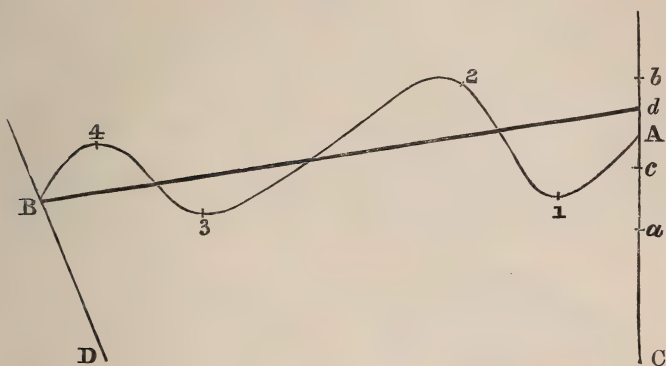
Having this triangle, it is easy to draw a rectangle equal to it.

Now let $A 1 2 3 4 B$ (fig. 5) be the curved boundary of a field of which $A C$, $B D$ are fixed boundaries; it is required to find a straight boundary to replace the curved one through A and B .

Place the divided limb roughly parallel to $A C$, with its bevelled edge nearest to $A C$, and at a distance about equal to the breadth of either limb from it, and fix the points firmly in the paper. Now move the undivided limb till it cuts $A C$ at A and $1 2$ at 2 ; slide it along till it is on the point 1 , and mark the point a where it meets $A C$. Move it till it is against a and 3 , and slide it along till it is against 2 , marking the point b where it meets $A C$ produced, if necessary. Move it till it is on b and 4 , slide it till it is on 3 , and mark c ,

the point where it meets AC. Move it on to *c* B, slide back to 4, and mark point *d* where it meets AC; join *d* B. *d* B is

Fig. 5.



the line required, as may easily be seen by comparing the portions given and taken by the respective fields. This line would only divide the fields perfectly accurately as before, if A 1 2, 2 3 4, &c., were rectilineal angles; but it is the nearest approximation possible, and the one always used.

The divider would also be useful on board ships for drawing lines on charts parallel to other lines at some distance. It would be much better for this purpose than an ordinary parallel ruler on account of its very large range and capability of being fixed.

Of course the line-divider can be used in all cases in which parallel rulers are ordinarily used.

In summing up, the advantages of the line-divider appear to be these:—

1. Lines of division can be drawn at any angle to the line to be divided.
2. Areas can be divided by equidistant lines parallel to any given line.
3. Since the divisions on the instrument are all equal, they may conveniently be made inches and parts of an inch; and
- 4, consequently the number of divisions may be made as great as we like without materially increasing the cost.
5. Lastly, the cost of the instrument is small, and it is made by Messrs. Stanley in three sizes—6, 12, and 24 inches in length.

XXXII. *The Logical Spectrum.*

By A. MACFARLANE, M.A., D.Sc., F.R.S.E.*

IN my work on the Algebra of Logic I investigated the meaning of Boole's analysis by the aid of diagrams. In this paper I propose to make a further use of the same method.

When the class of things considered (that is, the universe) is subdivided by not more than three qualities, a modified use of Euler's circles is sufficient. Let a square represent the universe of objects considered, and let a circle separate those having a particular quality. Diagram 1 represents the two classes formed by the presence and absence of the mark a ; and diagram 2 represents each of the four classes ab , ab' , $a'b$,

Diagram 1.

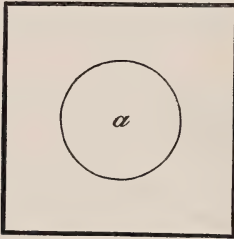
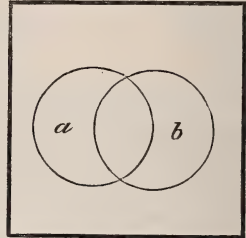


Diagram 2.



$a'b'$ formed by the presence or absence of each of the two marks a and b . Here the dash is used to denote *non*, as a' for non- a . It is a contraction for the complementary expression $1-a$. The diagram 3 is quite general, because it represents each of the eight classes abc , abc' , $ab'c$, $ab'c'$, $a'bc$, $a'bc'$, $a'b'c$, $a'b'c'$ formed by the three marks a , b , and c . But if we

Diagram 3.

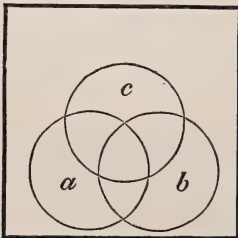
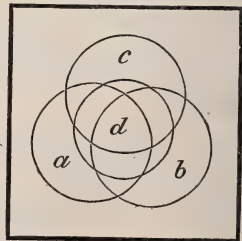


Diagram 4.



attempt by means of four circles to represent each one of the sixteen possible classes formed by four marks a , b , c , d , we shall find that it is impossible. For example, in diagram 4

* Communicated by the Author.

two possible classes are not represented ; hence the diagram is not general.

Another method, which I propose to call the *logical spectrum*, is capable of representing quite generally the universe subdivided by any number of marks. Let the universe be represented by a rectangular strip, as in diagrams 5 to 8.

Diagram 5.



Diagram 6.

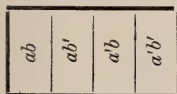


Diagram 7.

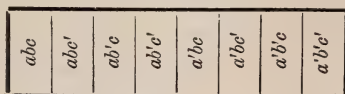
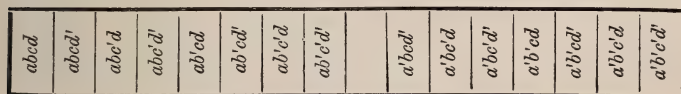


Diagram 8.



By subdividing into two we represent the possible classes formed by one mark a ; by subdividing each of these parts again we represent the four possible classes formed by two marks a and b ; and by subdividing each of these four parts again we represent the eight possible classes formed by the three marks a, b, c ; and so on. This method allows all the a part to be contiguous ; but the b part is broken up into two portions, the c part into four portions, the d part into eight portions. However, the regularity of the spectrum enables us easily to find all the portions belonging to any one mark.

I shall apply this method to verify the solution of the logical equations

$$U \{ax + by = c\},$$

$$U \{dx - ey = f\}.$$

Here U is the symbol for the whole of the objects considered ; it corresponds to the strip of paper in the diagram. The letters a, b, c, d, e, f represent known marks. There are two unknown marks denoted by x and y , which are such that the part of U having the marks a and x , together with the part having the marks b and y , is identical with the part having the mark c ; and the part having the marks d and x , excepting the part having the marks e and y , is identical with the part having the mark f . It is required to select the part of U which has the mark x , and also the part which has the mark y .

By the ordinary process of solution of simultaneous equations we get

$$x = \frac{ce + bf}{ae + bd} \quad \text{and} \quad y = \frac{cd - af}{ae + bd}.$$

Now, according to Boole's method,

$$\begin{aligned} \frac{ce + bf}{ae + bd} = & A_1 abcdef + A_2 abcdef' + A_3 abcde'f + A_4 abcde'f' \\ & + \quad \quad \quad + \quad \quad \quad + \quad \quad \quad + \\ & + A_{61} a'b'c'd'ef + A_{62} a'b'c'd'ef' + A_{63} a'b'c'd'e'f \\ & \quad \quad \quad + A_{64} a'b'c'd'e'f'; \end{aligned}$$

where the coefficients A_1, A_2, \dots, A_{64} are numerical. The coefficient for any term is found by supposing that term coextensive with the universe, and substituting 1 or 0, as the case may be, for each of the letters in $\frac{ce + bf}{ae + bd}$. If $U abcdef$ is identical with the universe, then a, b, c, d, e, f are each 1, and

$$A_1 = \frac{1+1}{1+1} = 1.$$

If $U abcdef' = U$, then f is 0 and all the others 1, and

$$A_2 = \frac{1}{2}.$$

According to Boole, these coefficients are susceptible of one or other of four interpretations: 1 means *all*, 0 means *none*, $\frac{0}{0}$ means *none* or *a portion* or *all*, and every other coefficient shows that the term is impossible.

When I sought to determine the coefficients for x and y by the above method, I found that many coefficients assumed the indeterminate form; and I found, on verifying the solution by means of the logical spectrum, that some of those terms could not be really indeterminate. Eventually I discovered a simpler method of finding the coefficients; and the solution obtained by it may be verified by the spectrum. It consists in substituting the special values of a, b, c, d, e, f (due to supposing a particular term identical with the universe) in the original equations, and then solving for x and y . The values so found are the coefficients for the terms.

For the first term $abcdef$ we get

$$x + y = 1, \quad x - y = 1;$$

from which

$$x=1 \text{ and } y=0;$$

hence

$$A_1=1 \text{ and } B_1=0.$$

For the second term $abcdef'$ we get

$$x+y=1, \quad x-y=0;$$

hence y is complementary to x and also identical with x , which is impossible unless the term $abcdef'$ is null.

For the fifth term $abcd'ef$ we get

$$x+y=1, \quad -y=1;$$

but y cannot be negative unless $abcd'ef$ is null.

For the seventh term,

$$x+y=1, \quad 0=1;$$

but the latter equation is impossible unless the term vanish.

For the eighth term,

$$x+y=1, \quad 0=0;$$

hence x and y are indeterminate, but complementary to one another. In the case of the 58th term, we get x and y both indeterminate but identical with one another. These are interpretations which are not conceived of by Boole.

For the nineteenth term,

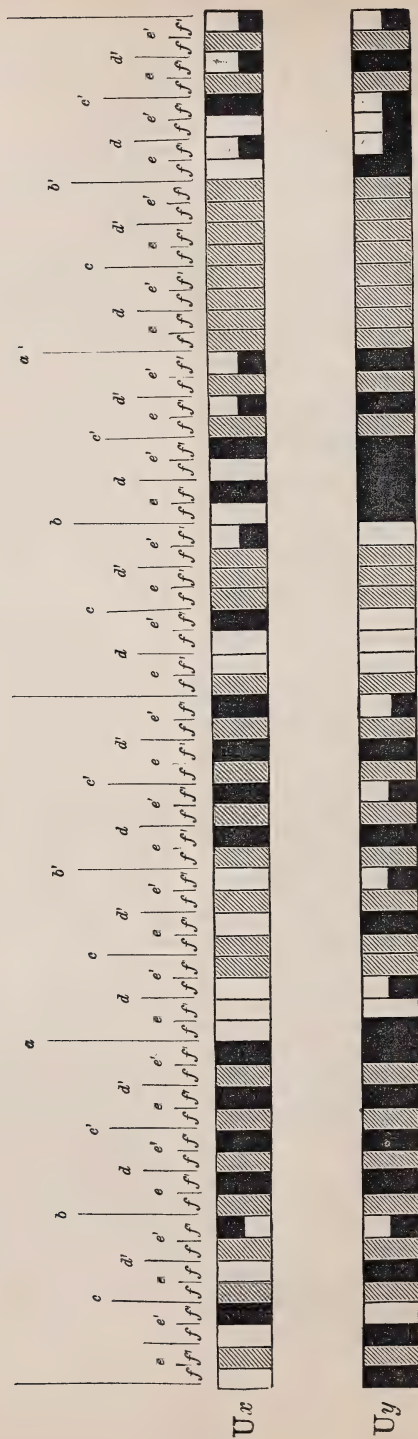
$$x=1, \quad x=1;$$

hence A_{19} is 1 and B_{19} is indeterminate, that is may have any value between 0 and 1. In a similar manner, by solving each of the sixty-four sets of equations the several coefficients are found. In the last case only are both coefficients indeterminate and independent of one another.

I have exhibited the solution obtained by this analysis in the accompanying spectrum (diagram 9). Each null term is shaded; each term which is wholly included is white; each term which is totally excluded is black; and each term which is indeterminate is partly black and partly white. The two complementary indeterminates have complementary parts white, and the two identical indeterminates have the same parts white.

The solution is verified by finding whether the ax together with the by is identical with the c , and whether the dx , excepting the ey , is identical with the f ; and by testing whether any other solution not comprehended as a particular case of that obtained would satisfy the conditions.

Diagram 9.



XXXIII. *On the Quadrant-Electrometer.*By J. HOPKINSON, *D.Sc., F.R.S.**

IN Professor Clerk Maxwell's 'Electricity' (vol. i. p. 273) it is proved that the deflection of the needle of a quadrant-electrometer varies as $(A-B)\left(C-\frac{A+B}{2}\right)$, where C is the potential of the needle, and A and B of the two pairs of quadrants. Desiring to ascertain the value of the standard charge of my instrument, I endeavoured to do so by the aid of this formula, and also by a more direct method. The results were quite discordant. Setting aside the special reasoning by which the formula is attained, we should confidently expect that the sensibility of a quadrant-electrometer would increase continuously as the charge of the jar is increased, until at last a disruptive discharge occurs. In my instrument this is not the fact. As the charge was steadily increased by means of the replenisher, the deflection of the needle due to three Daniell's elements at first increased, then attained a maximum, and with further increase of charge actually diminished. On turning the replenisher in the inverse direction the sensibility at first increased, attained the maximum previously observed, and only on further reduction of charge diminished.

Before giving the experimental results, it may be worth while to briefly examine the theory of the quadrant-electrometer. Let A, B, C, D be the potentials of the quadrants, the needle, and the inductor which is used for measuring high potentials (see Reprint of Sir W. Thomson's papers, p. 278). Let Q_1, Q_2, Q_3, Q_4 be the quantities of electricity on these bodies respectively, and θ the angle of deflection of the needle, measured in terms of divisions of the scale, on which the image of the lamp-flame is projected. We have the equations

$$\left. \begin{aligned} Q_1 &= q_{11}A - q_{12}B - q_{13}C - q_{14}D, \\ Q_2 &= -q_{12}A + q_{22}B - q_{23}C - q_{24}D, \\ Q_3 &= -q_{13}A - q_{23}B + q_{33}C - q_{34}D, \\ Q_4 &= -q_{14}A - q_{24}B - q_{34}C + q_{44}D. \end{aligned} \right\} \dots (1)$$

q_{11} &c. are the coefficients of capacity and induction. They are independent of A, B, C, D , and are functions of θ only. As above written, they are all positive. Let the energy of electrification be W :—

* Communicated by the Physical Society, having been read at the Meeting on March 14th, 1885.

$$\left. \begin{aligned} 2W &= q_{11}A^2 + q_{22}B^2 + q_{33}C^2 + q_{44}D^2 \\ &\quad - 2q_{12}AB - 2q_{13}AC - 2q_{14}AD \\ &\quad - 2q_{23}BC - 2q_{24}BD \\ &\quad - 2q_{34}CD \end{aligned} \right\} . . . \quad (2)$$

Equations (1) and (2) are perfectly general, true whatever be the form of the four bodies.

If the four quadrants completely surround the needle,

$$\left. \begin{aligned} q_{34} &= 0, \\ q_{14}, q_{24}, \text{ and } q_{44} &\text{ are independent of } \theta, \\ q_{33} &= q_{13} + q_{23}. \end{aligned} \right\} . . . \quad (3)$$

Now when the electrometer is properly adjusted, the needle will not be deflected when $A=B$, whatever C and A may be.

Hence $A-B$ is a factor of $\frac{dW}{d\theta}$, and we have

$$\left. \begin{aligned} \frac{dq_{11}}{d\theta} + \frac{dq_{22}}{d\theta} &= 2 \frac{dq_{12}}{d\theta}, \\ \frac{dq_{33}}{d\theta} &= 0; \end{aligned} \right\}$$

whence

$$2 \frac{dW}{d\theta} = (A-B) \left(A \frac{dq_{11}}{d\theta} - B \frac{dq_{22}}{d\theta} - C \frac{dq_{13}}{d\theta} \right).$$

This should be true of any electrometer having the above adjustment correctly made.

But by suitably forming the three bodies A, B, C , further relations between the coefficients may be obtained. The condition of symmetry would give us $\frac{dq_{11}}{d\theta} = -\frac{dq_{22}}{d\theta}$; but it is not necessary to assume symmetry. If the circumferential termination of the needle be a circle centre in the axis of suspension (at least near the division of the quadrants), if the needle turn in its own plane, if the quadrants are each approximately a surface of revolution about the axis, and if the radial terminations of the needle be not within the electrical influence of the quadrants within which they are not, conditions closely satisfied in Sir W. Thomson's electrometer,

$$\begin{aligned} \frac{dq_{11}}{d\theta} &= \frac{1}{2} \frac{dq_{13}}{d\theta}, \\ \frac{dq_{22}}{d\theta} &= -\frac{1}{2} \frac{dq_{13}}{d\theta}. \end{aligned}$$

If θ be small, we obtain

$$2 \frac{dW}{d\theta} = \alpha(A-B) \left(C - \frac{A+B}{2} \right),$$

the formula in Maxwell.

Returning now to our original equation, we have

$$\begin{aligned} 2W = & q_{11}A^2 + q_{22}B^2 + q_{33}C^2 + q_{44}D^2 \\ & - 2q_{12}AB - q_{33}AC - 2q_{14}AD \\ & - q_{33}BC - 2q_{24}BD \\ & + \alpha\theta(A-B) \left(C - \frac{A+B}{2} \right); \end{aligned}$$

involving in all eight constants, q_{11} &c. being now regarded as representing the values of the coefficients in the zero position.

$$\left. \begin{aligned} Q_1 = & q_{11}A - q_{12}B - \frac{1}{2}q_{33}C - q_{14}D + \alpha\theta(C-A), \\ Q_2 = & -q_{12}A + q_{22}B - \frac{1}{2}q_{33}C - q_{24}D - \alpha\theta(C-B), \\ Q_3 = & -\frac{1}{2}q_{33}A - \frac{1}{2}q_{33}B + q_{33}C + \alpha\theta(A-B), \\ Q_4 = & -q_{14}A - q_{24}B + q_{44}D. \end{aligned} \right\}$$

We may now discuss a variety of important particular cases.

(a) B is put to earth; A then is connected to a condenser, capacity a , charged to potential V : we want to know V from the reading of the electrometer. Here

$$\begin{aligned} \alpha V - \frac{1}{2}q_{33}C &= (a + q_{11})A - \frac{1}{2}q_{33}C + \alpha\theta(C-A), \\ V &= A + \frac{q_{11}}{a}A + \frac{\alpha\theta(C-A)}{a}. \end{aligned}$$

Neglecting A compared with C, and assuming

$$\theta = \lambda(A-B) \left(C - \frac{A+B}{2} \right),$$

we have

$$V = A \left\{ 1 + \frac{q_{11}}{a} + \frac{\lambda\alpha C^2}{a} \right\}.$$

The apparent capacity of A increases with C.

(b) B is again zero. A is connected to a source, but is disconnected and insulated when the deflection of the needle is θ' ; the final deflection is θ : required the potential V of the source.

$$\begin{aligned} q_{11}V - \frac{1}{2}q_{33}C + \alpha\theta'(C-V) &= q_{11}A - \frac{1}{2}q_{33}C + \alpha\theta(C-A), \\ V &= A + \frac{\alpha(\theta - \theta')C}{q_{11}} \\ &= A \left\{ 1 + \frac{\alpha\lambda \left(1 - \frac{\theta'}{\theta} \right) C^2}{q_{11}} \right\}. \end{aligned}$$

We may now consider the methods of varying the sensibility of the instrument (see Reprint of Sir W. Thomson's papers, p. 280). The methods dealt with are those of Sir W. Thomson, somewhat generalized.

(c) The quadrant B is connected with an insulated condenser, capacity b , whilst A is connected to a source of electricity:—

$$0 = -q_{12}A + (b + q_{22})B - \alpha\theta C,$$

$$\theta = \lambda(A - B)C;$$

therefore

$$0 = -q_{12}A + (b + q_{22})\left(A - \frac{\theta}{\lambda C}\right) - \alpha\theta C;$$

so

$$A = \theta \frac{\frac{b + q_{22}}{\lambda C} + \alpha C}{(b + q_{22}) - q_{12}}.$$

If $b=0$, we have the first reduced sensibility given by Sir W. Thomson.

(d) All methods of using the inductor may be treated under one general form. Let the quadrants A and B be connected with insulated condensers, capacities a and b ; then connect the inductor to a source, potential V ;

$$\left. \begin{aligned} 0 &= (q_{11} + a)A - q_{12}B - q_{14}V + \alpha\theta C, \\ 0 &= -q_{12}A + (q_{22} + b)B - q_{24}V - \alpha\theta C, \\ 0 &= \lambda(A - B)C; \end{aligned} \right\}$$

$$\begin{aligned} & (+q_{11}q_{22} - q_{12}^2 + aq_{22} + bq_{11} + ab)(A - B) \\ & + \{q_{14}(q_{12} - q_{22} - b) - q_{24}(q_{12} - q_{11} - a)\}\theta V \\ & + \{-2q_{12} + q_{22} + q_{11} + b + a\}\alpha\theta C = 0; \end{aligned}$$

whence we have an expression for V proportional to θ . By a proper choice of a and b , we can make the sensibility as low as we please.

Now the whole of these formulæ rest on the same reasoning as the equation

$$\theta = \lambda(A - B)\left(C - \frac{A + B}{2}\right).$$

I have mentioned that, in my instrument at least, this equation quite fails to represent the facts when C is considerable. It becomes a matter of interest to ascertain when the formula begins to err to a sensible extent. If a constant battery of a large number of elements were available, this would be soon accomplished. I have at present set up only 18 Daniells. I have therefore been content to use the electrometer to ascertain its own charge

by the aid of the inductor, using the 18 Daniells as a standard potential. As the charges range as high as 2600 Daniell's elements, the higher numbers can only be regarded as very rough approximations; sufficiently near, however, to indicate the sort of result which would be obtained if more precise methods were used. The first column in the following Table gives the ascertained or estimated charge of the jar of my electrometer in Daniell's elements; the second the deflection in scale divisions caused by three elements; the third, the coefficient λ , deduced by the formula $\theta = \lambda AC$: this coefficient ought theoretically to be constant.

I.	II.	III.
72	75	0.35
112	118	0.35
136	140	0.35
178	190	0.35
238	239	0.34
303	288	0.32
383	336	0.30
512	391	0.26
616	409	0.22
813	432	0.18
1080	424	0.13
1312	402	0.10
1728	360	0.07
2124	320	0.05
2634	296	0.037
1704	353	0.07
1436	394	0.09
1284	412	0.11
876	436	0.17
684	427	0.21

By connecting the jar and one quadrant to 18 elements and the other quadrant to earth, I obtained 0.356 as the value of λ , making use of the complete equation

$$\theta = \lambda(A - B) \left(C - \frac{A + B}{2} \right).$$

It will be seen that this equation may be trusted until C is over 200 Daniell's elements potential, but that when C exceeds 250 a quite different law rules.

The foregoing was read before the Physical Society a few years ago, but I stopped its publication after the type was set up, because I was not satisfied that my appliances for experiment were satisfactory, or that I could give any satisfactory explanation of the anomaly.

The electrometer had been many times adjusted for various purposes before further experiments were made, so that those

which I shall now describe cannot be directly compared with what goes before. The old experiment was first repeated, and the existence of a maximum sensibility again found. On examination, it was found that the needle hung a little low so that it was nearer to the part of the quadrant below it than to that above. It is easy to see that this would produce the anomalous result observed, though there is reason for thinking it is not the sole cause. The effect of the needle being low is that it will be on the whole attracted downwards; and so the apparent weight hanging on the fibre-suspension and the consequent tension of the fibres will be increased. The increase of the tension will be as the square of the potential C ; and hence the formula for the deflection will be modified to

$$\theta = \frac{\lambda}{1 + kC^2} (A - B) \left(C - \frac{A + B}{2} \right),$$

where k is a constant depending upon the extent to which the position of the needle deviates from its true position of midway between the upper and lower parts of the quadrants. By a proper choice of k , the results I previously obtained are found to agree well with this formula.

The electrometer was next adjusted in the following way:—The needle was raised by taking up the fibres of the suspension and adjusting them to equal tension in the usual way, and the proportionality of sensibility to charge was tested, the charge being now determined in arbitrary units by discharging the jar of the instrument through a ballistic galvanometer. The operation was repeated until the sensibility, so far as this method of testing goes, was proportional to the charge of the jar over a very long range. It was then found that the needle was slightly above the median position within the quadrants. Increased tension of the fibres from electrical attraction does not therefore account for the whole of the facts, although it does play the principal part. The sensibility of the instrument being now at least approximately proportional to the charge of the jar, I proceeded to determine accurately the potential of the jar when charged to the standard as indicated by the idiostatic gauge.

In what follows the quadrants, one of which is under the induction-plate, are denoted by B , the others by A . The quadrants B are connected to the case, A are insulated. The jar is connected to the induction-plate, and the reading on the scale noted; the connection is broken, and the induction-plate is connected to the case, and the reading on the scale again noted; the difference is the deflection due to the charge in the jar. It is necessary to read the scale for zero-charge

on induction-plate last, because the charging of the induction-plate slightly diminishes the charge of the jar, and considerably displaces the zero-reading by giving an inductive charge to the quadrant A. It is also necessary to begin with the charge of the jar minutely too high, so that after separating the induction-plate from the interior of the jar, the latter shall have exactly the correct charge as indicated by the gauge. The deflection thus obtained was precisely $298\frac{1}{2}$, repeated in many experiments. The *double* deflection given by seventy Daniell cells was 43·6 scale-divisions. By comparison with two Clark's cells, the value of which I know, the potential of the seventy Daniells was found to be 74·2 volts; hence the potential of the jar is 1016 volts, when charged to the potential indicated by the gauge.

The constant λ of the instrument was next determined by the formula

$$\theta = \lambda(A - B)\left(C - \frac{A + B}{2}\right).$$

Four modes of connecting are available for this :—

$$A = C = 74\cdot2 \text{ volts, } B = 0 ;$$

$$B = C = 74\cdot2 \text{ volts, } A = 0 ;$$

$$A = C = 0, B = 74\cdot2 \text{ volts ;}$$

$$B = C = 0, A = 74\cdot2 \text{ volts.}$$

In each case the deflection was 253·5 if the charge on the needle was positive in relation to the quadrant with which it was not connected; and was 247 when the needle was negative. This at first appeared anomalous; but the explanation is very simple. The needle is aluminium, the quadrants are either brass or brass-gilded, I am not sure which. There is therefore a contact-difference of potential between the needle and the quadrants; call it x . Thus, instead of $\theta = \frac{A^2}{2}$, we have

$$\theta = +\lambda A\left(\frac{A}{2} + x\right)$$

and

$$\theta = \lambda(-A)\left(-\frac{A}{2} + x\right);$$

this gives

$$x = \frac{6\cdot5}{2\lambda A} = 0\cdot482 \text{ volt.}$$

The result was verified by using fourteen cells instead of seventy: the deflections were 10·0 and 8·8, which gives the same value to x . It is worth noting that the same cause

affects the idiostatic gauge in the same way. Let the jar be charged till the gauge comes to the mark. Call P the difference of potential between the aluminium lever of the idiostatic gauge and the brass disk below which attracts it. The difference of potential between the brass of the case and the brass work of the interior is $P + x$, and between the case and the aluminium needle within the quadrants it is $P + 2x$. If, however, the charge is negative, the difference is $-P + 2x$. Hence the sensibility will be different from two causes, according as the jar is charged positively or negatively, till the idiostatic gauge is at its standard. For determining the constant λ we must take the mean of the two results 253.5 and 247, that is 250.25. Comparing this with the actual standard charge of the jar, and the double deflection given by one volt 172.4, when charged to the standard, we see that the irregularity has not been wholly eliminated. It appeared desirable to determine the sensibility of the instrument for a lower known charge. The charge was determined exactly as described above, and was found to be 609 volts; whilst 1 volt gave 107.1 scale-divisions double deflection; whence in the equation

$$\theta = \frac{\lambda(A-B)C}{1+kC^2},$$

we have, if

$$\lambda = 0.1816, \quad k = 7 \times 10^{-8},$$

the following as the calculated and observed deflections:—

Calculated	250.0,	107.7,	172.4
Observed	250.2,	107.1,	172.4,

which is well within errors of observation.

This deviation from proportionality of sensibility did not appear to be worth correcting, as I was not sure that other small irregularities might not be introduced by raising the needle above the middle position within the quadrants. It appears probable that the small deviation still remaining does not arise from the attraction of the quadrants on the needle increasing the tension of the suspension, but from some cause of a quite different nature, for if it were so caused the capacity-equations would be

$$\theta_1 = q_{11}A - q_{12}B - q_{33}C - q_{14}D + \alpha\theta C, \text{ \&c.,}$$

where

$$\theta = \frac{\lambda}{1+kC^2}(A-B) \left(C - \frac{A+B}{2} \right).$$

Now the experiments I have tried for determining q_{11} , q_{12} ,

&c., are not in accord within the limits of errors of observation, using these equations of capacity; but they are in better accord if, in lieu of the term $\alpha\theta C$, we write $\frac{\alpha\theta C}{1+kC^2}$. I have no explanation of this to offer; but in what follows it is assumed that the equations expressing the facts are

$$\left. \begin{aligned} \theta &= \mu(A-B), \text{ where } \mu = \frac{\lambda C}{1+kC^2}, \\ Q_1 &= q_{11}A - q_{12}B - q_{14}D + \beta\mu\theta, \\ Q_2 &= -q_{12}A + q_{22}B - q_{24}D - \beta\mu\theta, \\ Q_4 &= -q_{14}A - q_{24}B + q_{44}D. \end{aligned} \right\}$$

We are now in a position to determine the various coefficients of capacity: in doing so it is necessary to distinguish the values of q_{11} and q_{22} when the posts by which contact with the quadrants is made are down and in contact with the quadrants, and when they are raised up out of contact; the former are denoted by $q_{11}+a$ and $q_{22}+a$, the latter by q_{11} and q_{22} , the capacity of the binding-posts being a . As a convenient temporary unit of capacity the value of $\beta\mu^2$, when the jar has the standard charge, is taken. The first set of experiments was to determine the deflections caused by known potentials with varied charge of jar, one or other of the quadrants being insulated. Three potentials of the jar were used—that of the standard indicated by the idiostatic gauge and two lower. The values of μ are denoted by μ_3, μ_2, μ_1 . It was found by connecting the two quadrants to standard cells that

$$\mu_3 : \mu_2 : \mu_1 = 1 : 0.805 : 0.585 ;$$

and hence

$$\beta\mu_3^2 = 1, \beta\mu_2^2 = 0.648, \beta\mu_1^2 = 0.342.$$

Suppose quadrant A be insulated, and potential B be applied to quadrant B; then we have, if θ be deflection which potential B would cause with standard charge, if quadrant A were connected to the case, and ϕ the observed deflection,

$$\begin{aligned} 0 &= q_{11}A - q_{12}B + \beta\mu\phi ; \\ \phi &= \mu(A-B) ; \\ \theta &= -\mu_3B ; \end{aligned}$$

whence

$$\phi = \theta \cdot \frac{\mu}{\mu_3} \cdot \frac{q_{11} - q_{12}}{q_{11} + \beta\mu^2}.$$

In the calculated values of ϕ given below,

$$\begin{aligned} q_{11} &= 0.502, & q_{22} &= 0.543 \\ q_{12} &= 0.293, & a &= 0.200 \text{ for B,} \\ & & &= 0.193 \text{ for A.} \end{aligned}$$

A closer approximation to observation is obtained by assuming the two contact-posts to be of slightly different capacities; the difference given above is no more than might be expected to exist.

The jar being charged to standard potential, B was insulated, and its post raised, and A was connected to 10 Daniells, for which $\theta = 1808$:—

Deflection observed = 293·2,

„ calculated = 293·0.

The post of B was lowered to contact :—

Deflection observed = 467·0,

„ calculated = 466·8.

A was now insulated and post raised, B was connected to the same battery :—

Deflection observed = 251·0,

„ calculated = 251·6.

The post of A was lowered to contact :—

Deflection observed = 429·0,

„ calculated = 428·8.

The jar was now charged to a lower potential, for which $\mu = \mu_2$ with B insulated and post raised, and A connected to 30 Daniells, for which $\theta = 5468$:—

Deflection observed = 925·0,

„ calculated = 924·0.

The post of B was lowered to contact, and A connected to 10 Daniells, for which $\theta = 1808$:—

Deflection observed = 470·5,

„ calculated = 470·85.

A was now insulated and post raised, B was connected to a battery of 30 Daniells, for which $\theta = 5468$:—

Deflection observed = 798·0,

„ calculated = 800·0.

The post of A was lowered to contact, and B was connected to 10 Daniells ; $\theta = 1808$:—

Deflection observed = 437·0,

„ calculated = 435·7.

The jar was then charged to a still lower potential, for which $\mu = \mu_1$, with B insulated and post raised, and A connected to 30 Daniells, for which $\theta = 5468$:—

Deflection observed = 901·0,

„ calculated = 903·6.

The post of B was lowered to contact and A connected to

10 Daniells ; $\theta = 1808$:—

Deflection observed = 437.0,

„ calculated = 438.7.

A was now insulated and post raised, and B was connected to 30 Daniells ; $\theta = 5468$:—

Deflection observed = 785,

„ calculated = 792.

The post of A was lowered to contact and B connected to 10 Daniells ; $\theta = 1808$:—

Deflection observed = 408,

„ calculated = 410.

The next experiment was similar, excepting only that the insulated quadrant B was connected to a condenser ; this condenser consisted merely of a brass tube insulated within a larger tube—its capacity is about 0.00009 microfarad. The jar was at its standard charge. Calling the capacity of the condenser b , in terms of our temporary unit, we have, as before,

$$\phi = \theta \frac{b + a + q_{22} - q_{12}}{b + a + q_{22} + 1} [\beta \mu_3^2 = 1].$$

When $\theta = 1259$, ϕ was observed to be 927, whence $b = 3.159$.

We are now in a position to obtain independent verification of the values already obtained for the constants. Suppose A be connected to the case, that condenser b is charged from a battery of known potential, such that it would give deflection θ if connected to B, and the charged condenser is then connected to B. Suppose ψ be the deflection before connection is made, ϕ after. Then

$$b(\theta - \phi) = \{q_{22} + a + 1\}(\phi - \psi).$$

When $\theta = 1439$ and $\psi = 0$, it was found that $\phi = 915$. The value of ϕ , calculated from the values of the constants already obtained, is 928.

When $\theta = 1439$ and $\psi = -676$, it was found that $\phi = +676$; the calculated value is 688.

A further experiment of verification, involving only the capacity of the quadrant, is the following. The quadrant A being connected to the case, B was charged by contact instantaneously made and broken with a battery of known potential, and the resulting deflection was noted. The instantaneous contact being made by hand, no very great accuracy could be expected. Let ψ and ϕ be the readings on the scale before and after the instantaneous contact ; then

$$\frac{\theta - \phi}{\phi - \psi} = \frac{1}{q_{22} + a} = 1.345.$$

The following results were obtained:—

θ .	ψ .	ϕ observed.	ϕ calculated.
1796	0	763	765
1796	-493	493	482

We next determine the coefficients q_{14} and q_{24} of induction of the induction-plate on the quadrants. This is easily done from the deflections obtained with the induction-plate, one or both pairs of quadrants being insulated. First, suppose one pair, say B, are insulated whilst A is connected to the case:—

$$0 = +q_{22} B - q_{24} D - \beta\mu\phi,$$

$$\phi = -\mu B,$$

$$\theta = \mu_3 D;$$

whence

$$\phi = \theta \frac{\mu}{\mu_3} \frac{q_{24}}{q_{22} + \beta\mu^2},$$

ϕ being the deflection actually observed, and θ that which the battery used would give if connected direct to the quadrants, the needle having the standard charge. When θ was 12,800 and $\mu = \mu_3$, ϕ was 418, whence $q_{24} = 0.0504$.

In the same way A being insulated but B connected to the case, ϕ was found to be 43.6, whence $q_{14} = 0.00508$.

Again, when both quadrants are insulated we have

$$0 = q_{11}A - q_{12}B - q_{14}D + \beta\mu\phi,$$

$$0 = -q_{12}A + q_{22}B - q_{24}D - \beta\mu\phi,$$

$$\phi = \mu(A - B),$$

$$\theta = \mu_3 D.$$

From the first two equations,

$$(q_{11}q_{22} - q_{12}^2)(A - B) - \{(q_{22} - q_{12})q_{14} - (q_{11} - q_{12})q_{24}\}D + (q_{22} + q_{11} - 2q_{12})\beta\mu\phi = 0;$$

whence

$$\phi = \theta \frac{\mu}{\mu_3} \frac{(q_{22} - q_{12})q_{14} - (q_{11} - q_{12})q_{24}}{(q_{11}q_{22} - q_{12}^2) + (q_{22} + q_{11} - 2q_{12})\beta\mu^2}.$$

In the case when $\mu = \mu_3$, substituting the values already determined, we have

$$\phi = \theta \times 0.0142;$$

it was observed with $\theta = 12,800$ that $\phi = 183$; the calculated value would be 182.

With a lower charge on the jar, viz. when $\mu = \mu_3 \times 0.805$, with B insulated, A connected to the case, and $\theta = 12,800$, it was found that $\phi = 437.5$; the calculated value is 441.

The capacity q_{44} of the induction-plate is of no use; its value, however, is about 0.004, in the same unit as has been so far used.

The capacity q_{33} of the needle and the coefficient of induction of the needle on either quadrant $\frac{1}{2}q_{33}$ are also of no use, but the method by which they may be obtained is worth noting. Let quadrants A be connected to the case, and let B be insulated, diminish the charge of the jar slightly by the replenisher, and suppose the consequent deflection be ϕ . Let μ and μ' be the values of μ before and after the diminution of charge, as ascertained by applying a known potential-difference between the two pairs of quadrants; we have

$$-q_{33}C = +q_{22}B - q_{33}C' - \beta\mu'\phi,$$

where

$$\phi = -\mu'B,$$

$$q_{33}(C - C') = (q_{22} + \beta\mu'^2)B,$$

which determine q_{33} , since C and C' are known from μ and μ' .

Of course the values of the constants of an electrometer are of no value for any instrument except that for which they are determined in the state of adjustment at the time. For any particular use of the instrument it is best to determine exactly that combination of constants which will be needed. Nor is there anything new in principle in the discussion or experiments here given; they are merely for the most part the application of well-known principles to methods of using the electrometer given by Sir William Thomson himself. The method of determining the capacity of a condenser by charging it and connecting it to an insulated quadrant has been used by Boltzmann. But the invention of the quadrant-electrometer by Sir W. Thomson may be said to have marked an epoch in Electrostatics, and the instrument from time to time finds new uses. It therefore seems well worth while to make known observations made upon it in which the instrument itself has been the only object studied. Some practical conclusions may, however, be drawn from the preceding experiments. Before using the formula

$$\theta = \lambda(A - B)\left(C - \frac{A + B}{2}\right)$$

it is necessary to verify that it is sufficiently nearly true, or to determine its variation from accuracy. Unless it be sufficiently accurate through the range experimented upon, the electrometer cannot be applied by the methods well known for determining alternating potentials and the work done by alternating currents.

My pupil, Mr. Paul Dimier, has very efficiently helped me in the execution of the experiments of verification.

XXXIV. *The most Economical Potential-difference to employ with Incandescent Lamps.* By Professors W. E. AYRTON, F.R.S., and JOHN PERRY, M.E.*

THE subject in connection with which the accompanying paper is a small contribution is one of considerable commercial importance. It has long been known that the luminous power of an electric lamp increased much more rapidly than the power electrically expended on it; or, that the number of candles per horse-power increased as the lamp was made to become brighter and brighter. Perhaps the earliest experiments on the subject were those published by Sir William Thomson in 1881, and those made by our students in 1880. Since that time (that is, during the last five years) tests of the efficiency of various types of incandescent lamps have been made over and over again by various persons, without perhaps its being clearly realized that such efficiency-experiments by themselves gave us no idea of the commercial value of any particular lamp.

It is not sufficient to know that when a lamp is giving out a certain number of candles it absorbs so much power per candle, and when giving out a much larger number of candles it absorbs so much less power per candle; but what must be known in addition is the life of the lamp at each of these two candle-powers, before we can decide whether it is more economical to use the lamp with the filament at not much more than a dull red or when brilliantly luminous with a bluish tint. For with the filament at a comparatively low temperature, although the efficiency of the lamp is low, its life will be great; whereas if the temperature of the filament be high, the large efficiency will be to some extent balanced by its short life and the consequent large cost due to lamp renewals.

In the 'Electrician' for January 31st of this year M. Foussat gives a table of values of the life of a 100-volt Edison lamp for different potential-differences; and M. Foussat has assured us that these numbers were obtained experimentally, and not from calculation according to any theory. That the results should lie so nearly in a curve is undoubtedly at first sight a little striking; but if it be remembered that they are stated to represent the results obtained from the *averages* of a very extended series of tests, their great regularity is not more striking than is a curve showing graphically the result of mortality tables. We asked M. Foussat whether he had also the results of experiments on the efficiency of the same type of 100-volt lamps, as our own experiments on the efficiency

* Communicated by the Physical Society, having been read at the Meeting on February 28th, 1885.

of Edison lamps had been made with 55-, 108-, and 110-volt lamps. As, however, he had no such results, and as his tests of lives are the only ones that we now have, and possibly may have, until the completion of the excellent work at present being carried out by the Electric-Lighting Jury of the Health Exhibition, we commenced this investigation by endeavouring to combine the lives given by M. Foussat with the results previously published for efficiency. To enable us to do this, we assumed that the life of 1000 hours given by M. Foussat for his 100-volt lamps when used with a potential-difference of 100 volts would be the same as that for our 108-volt lamp when employed with a potential-difference of 108 volts; or, in other words, we regarded lives given by him as applicable to our 108-volt Edison lamps when each of his potential-differences was multiplied by $\frac{108}{100}$.

To ascertain the most economic potential-differences, we must proceed as follows:—Let $f(v)$ be the life in hours as a function of the number of volts constantly kept on the lamps, $\theta(v)$ the number of candles emitted by one lamp as a function of the potential-difference in volts employed, let p be the price in pounds paid for one lamp, and n the number of hours per year that a lamp is kept burning; then

$$\frac{p \times n}{f(v) \times \theta(v)}$$

stands for the cost per year per candle, as far as the renewal of lamps is concerned.

Next, let H stand for the cost in pounds of an electric horse-power per year for the number of hours an electric horse-power is employed. It is often assumed that H is proportional to the number of hours per day that the power is used, or that the yearly bill for power should be based on the horse-power hours, or total energy consumed; but this idea, which runs even through the Electric Lighting Act, is quite an erroneous one. H will be of the form $h + F(n)$, where h is a constant independent of the number of hours and depending on the rent of the site, capital expended upon engines, dynamos, leads, &c.; and $F(n)$ is some function of the number of hours during which electric power is required, and depends on the cost of coal, superintendence, &c. If the light were only required for one or two hours in a district where rent was very high, h would be the all-important term and $F(n)$ would be unimportant; whereas if the electric power were required for various purposes, for, say, 15 or 20 hours out of the 24, in a place where rent was low but coal dear, $F(n)$ would be the important item in the yearly bill for electric power supplied.

Let $\phi(v)$ be the watts per candle, expressed as a function of the number of volts employed at the terminals of a lamp; then

$$\frac{H}{746} \times \phi(v)$$

represents the cost per year per candle as far as the production of electric power is concerned.

The total cost, therefore, per year per candle is

$$\frac{p \times n}{f(v) \times \theta(v)} + \frac{H}{746} \phi(v) \text{ pounds,} \quad . \quad . \quad . \quad (A)$$

and we must find the value of v that makes this a minimum.

There are two ways in which such a problem can be solved: the one a graphical method, the other an analytical method. The former may be used by even elementary students, and will be given first. It consists in drawing curves to represent, 1st, $f(v)$ in terms of v , 2nd, $\theta(v)$ in terms of v , and 3rd, $\phi(v)$ in terms of v ; and from these the values of $f(v)$, $\theta(v)$, and $\phi(v)$ are each determined graphically for many values of v , and the value of A calculated for each of these values. A fourth curve is then drawn, connecting the values of A with those of v , when it is easy to see by inspection for what value of v the expression A has a minimum value.

The following is the result so obtained for the 108-volt Edison lamps used for lighting the Finsbury Technical College:—

p is taken at 5s., or £0.25;

n as 560 hours, the time per year, approximately, during which the lamps are lighted;

H as £5: this is perhaps a rather high estimate for the cost of power, considering that the interest on the steam-engine, dynamos, &c., price of coal burnt, wages of the engine-driver and stoker have to be mainly debited to the driving of the College workshops, supplying power for the dynamos worked for experimental purposes; but it will be accurate enough to take the sum of £5 per year as representing yearly interest on extra plant and the yearly interest on the extra cost of supplying one electric horse-power during the 560 hours.

Then the value of v which makes A a minimum turns out to be about **106 volts**; and on account of the flatness of the curve connecting the expression A with v , we see that in this particular case the annual cost of supplying light is only increased by 3.5 per cent., if the potential-difference between the mains be kept diminished to about 104.8 or kept up to about 108 volts. Also, that keeping the potential-difference diminished to about 104.5, or increased to about 108.5 volts, increases our total annual cost of lighting by 5 per cent.

More recently one of our students, Mr. Robertson, has been making efficiency-experiments on some Edison lamps obtained from France; and by trial he has found some that give 16 candles at about 100 volts, and are therefore presumably of the same type as those employed in the life-experiments given by M. Foussat in the 'Electrician' for January 31st.

Mr. Robertson's results from one such lamp are as follow:—

TABLE I.

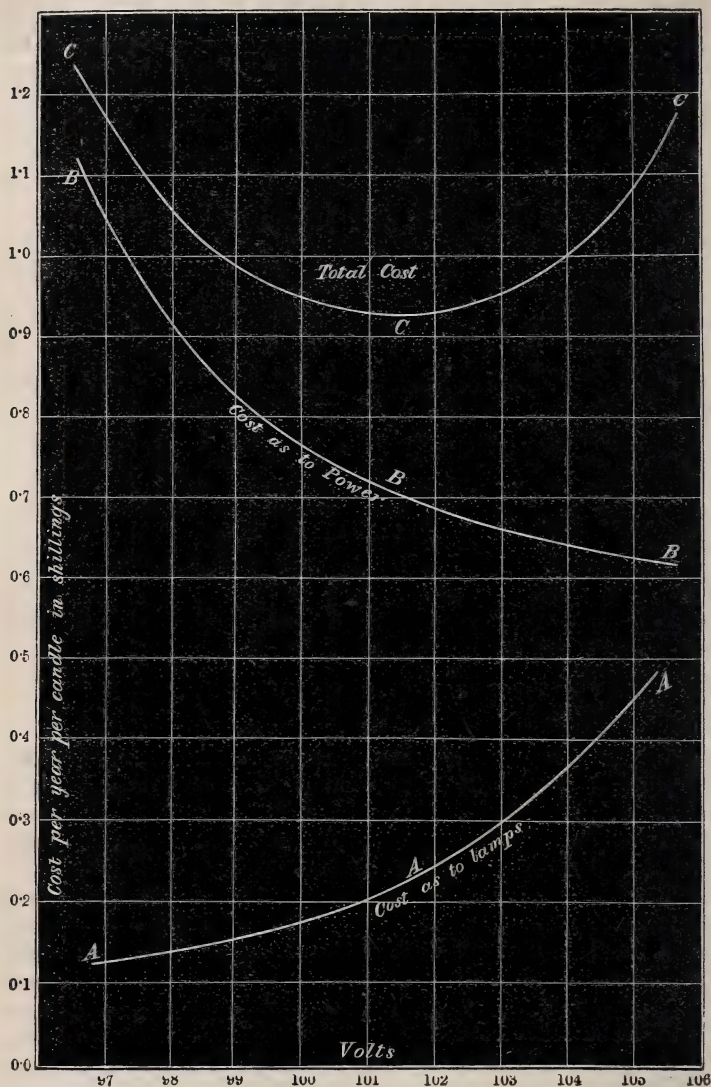
Candles, or $\theta(v)$.	Volts, or v .	Watts.
3	84	56.62
4	87	63.51
5	87.8	66.03
6	90.5	69.05
7	93.5	75.74
10	96.75	79.34
11	97.61	81.01
12	98.04	82.35
14	98.9	85.55
16	100.6	90.54
18	101.5	92.37
20	103.6	96.66
22	105.4	100.10
24	107.5	104.28
26	109.2	108.11
28	110	111.10
30	111.4	113.63
35	114	118.56
40	115.2	122.11

We are therefore now in a position to take up the problem in a more direct manner, without making any assumption beyond this—that Mr. Robertson's 16-candle 100-volt French Edison lamp is of the same kind as M. Foussat's 16-candle 100-volt French Edison lamp.

Solving the problem graphically, in the way previously described, and using the same values of p , n , and H —viz. cost of a lamp 5s., number of hours of burning per year 560, and annual cost of an electric horse-power for those 560 hours £5,—we obtain the curve A A A to represent the cost per candle per year as regards renewal of lamps, B B B the cost per candle per year as regards power, and the resultant curve C C C as the total cost per candle per year.

The minimum value of this cost appears from the curve C C C to be at about 101.4 volts, and to equal about 11d. per candle per year. If the potential-difference be maintained constantly down at 98.7 volts, or up at 104 volts, then the cost becomes 1s. per candle per year. It will also be seen from the curves that the yearly cost for renewals of lamps is

Diagram for a 16-candle 100-volt Edison Lamp.



but a small fraction of the total yearly cost, as long as we are using potential-differences of not more than about 100 volts; so that when using, with these lamps, this or a lower potential-difference, the actual sum paid for the lamps is not so very serious an item in the yearly lighting bill. But if, on the other hand, we maintain a potential-difference of, say, 104

volts constantly at the terminals, then the lamp renewals represent, in our particular case, nearly one third of the yearly expenditure, and consequently any change in the price of lamps becomes extremely important.

We find that the simplest expression for the candle-power of this incandescent lamp in terms of volts is

$$\theta(v) = a(v-b)^3. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Thus, for example, it will be found that if the cube roots of the values of $\theta(v)$, given in the preceding table, are plotted with the corresponding values of v as coordinates of points on squared paper, the points lie in no regular curve so nearly as in a straight line, discrepancies being apparently due to errors of observation, and these errors of observation follow a periodic law which may be of interest physiologically. Or it may be that there really is a point of inflexion in the curve.

For the particular lamp in question we find that

$$a = 0.0002621,$$

$$b = 62.12;$$

or, if the law remains true for a less number of volts than 84, the lowest used in this experiment, then when $v = 62.12$, the candle-power is zero.

We do not, however, find that the cube roots of the candle-powers of all sorts of incandescent lamps follow a line-function of the difference of potential at their terminals; for on examining the results of experiments that have been made with various types of lamps, we find that with the first five of the following set of lamps the law

$$\sqrt[3]{\theta(v)} \propto v - b$$

is very nearly true.

1. A Lane-Fox lamp from 50 to 80 volts.
2. A British Electric Light Co.'s lamp " 50 to 68 "
3. An Edison " 7 B " 8-candle lamp . " 45 to 63 "
4. An old Swan lamp " 40 to 64 "
5. A low-resistance 8-candle Swan lamp " 16 to 40 "

Whereas with the next five lamps it is not so true, although for half the range the law might be regarded as true in all the cases.

6. A Maxim lamp from 42 to 57 volts.
7. A Maxim lamp " 35 to 110 "
8. An old form Swan lamp " 25 to 54 "
9. A high-resistance Swan lamp . . . " 45 to 90 "
10. An Edison B 8-candle lamp . . . " 36 to 85 "

We may here mention that we have found the following plan very useful in helping us to draw correctly a curve at a place

where there is a sudden bend and an absence of points, determined from experiments to guide us in drawing the curve. Instead of plotting x and y , and obtaining a curve which it would be very difficult to draw correctly, we may by using some simple function of y obtain points which obviously lie in a curve which it is easy to draw. Thus we may plot \sqrt{y} and x , or $\sqrt[3]{y}$ and x , or $\log y$ and x . When the curve connecting y and x is not roughly asymptotic to the axis of x , but to some line parallel to this axis, it is obvious that there is a greater likelihood of obtaining simple curves by plotting $\sqrt{y \pm \alpha}$ and x , or $\sqrt[3]{y \pm \alpha}$ and x , or $\log(y \pm \alpha)$ and x , where α is some constant obtainable by inspection, than by simply plotting the function of y alone.

The following table gives M. Foussat's lives in terms of v and the corresponding values which we have calculated of $\log f(v)$, $\log \theta(v)$, and of $\log f(v)\theta(v)$ from the values of $\theta(v)$ given in the previous table.

TABLE II.

v .	$f(v)$.	$\log f(v)$.	$\log \theta(v)$.	$\log f(v)\theta(v)$.
95	3595	3.5557	0.9729	4.5286
96	2751	3.4395	1.0119	4.4514
97	2135	3.3294	1.0485	4.3779
98	1645	3.2161	1.0851	4.3012
99	1277	3.1062	1.1199	4.2261
100	1000	3.0000	1.1556	4.1556
101	785	2.8949	1.1898	4.0847
102	601	2.7789	1.2216	4.0005
103	477	2.6785	1.2531	3.9316
104	375	2.5740	1.2852	3.8592
105	284	2.4533	1.3152	3.7685

When $\log f(v) \theta(v)$, and v are plotted as coordinates of points, these points are found to lie so nearly on a straight line that the formula

$$\frac{1}{f(v)\theta(v)} = 10^{0.07545v - 11.697} \quad \dots \quad (2)$$

is found to be true with considerable accuracy, and such a formula lends itself with great ease to calculation.

It is quite true that we might have obtained a still more accurate formula than (2) since, as published by Mr. Wright, one of the members of our class, in the 'Electrician' for February 21st, the logarithm of the life of a lamp is shown to be a line-function of the difference of potentials at which it is worked.

We may in fact with this particular type of lamp put

$$f(v) = 10^{14 - 0.11v} \quad \dots \quad (3)$$

with very great accuracy indeed.

Combining this with (1), we obtain as a more accurate formula,

$$\frac{1}{f(v)\theta(v)} = \frac{1}{a} 10^{0.11v-14}(v-b)^{-3}. \quad (4)$$

But unfortunately this more accurate formula does not lend itself to mathematical calculation, whereas that given in (2) is very suitable for this, and has a sufficient degree of accuracy for our purpose. In using (2) we are really using for the candle-power

$$\theta(v) = 10^{0.03455v-2.303} \quad (5)$$

instead of (1).

It will be also found that from 95 to 105 volts, the range of volts given in Table II., the values of $\phi(v)$, or watts per candle-power, when corrected for errors of observation, satisfy with considerable accuracy the equation

$$\phi(v) = 3.7 + 10^{8.007-0.07667v} \quad (6)$$

If, however, we take the whole range of values given in Table I., then it will be found that the equation

$$\phi(v) = 2 + 10^{4.424-0.03793v} \quad (7)$$

is better satisfied than (6).

The following Table III. gives the numbers we have actually employed in making these calculations.

TABLE III.

v .	$\sqrt[3]{\theta(v)}$. corrected.	$\theta(v)$ corrected.	Watts corrected.	Watts per candle-power $\phi(v)$.	$\log \{\phi(v)-2\}$.
84	1.4	2.744	56	20.408	1.2650
86	1.53	3.582	60.15	16.79	1.1700
88	1.66	4.574	64.25	14.27	1.0888
90	1.786	5.7	68.4	12.0	1.0000
92	1.914	7.0	72.6	10.37	.9227
94	2.044	8.55	76.85	9.0	.8451
96	2.175	10.28	81	7.882	.7695
98	2.3	12.167	85.15	7.0	.6990
100	2.428	14.24	89.3	6.271	.6305
102	2.556	16.69	93.5	5.6	.5563
104	2.682	19.29	97.8	5.07	.4871
106	2.81	22.188	101.9	4.593	.4138
108	2.94	25.412	106	4.172	.3369
110	3.065	28.79	110.1	3.824	.2610
112	3.195	32.61	114.25	3.504	.1772
114	3.32	36.594	118.3	3.233	.0910

Using (6) and (2), the total cost of one candle per year is

$$p n 10^{0.07545v-11.697} + \frac{H}{746} (3.7 + 10^{8.007-0.07667v}).$$

This is a minimum when

$$v = 110.66 + 6.574 \log \frac{H}{pn}.$$

Hence taking $p = 0.25$, $n = 560$, $H = £5$, we find

$$v = 101.15 \text{ volts.}$$

Using (7) and (2), we should find in the same way that the minimum is obtained when

$$v = 101.46 \text{ volts ;}$$

and these minimum values of v agree very closely with that previously determined graphically.

One very important problem in connection with incandescent lamps, and one that cannot yet be regarded as solved, is the determination of the life of a lamp for any given number of volts, from experiments made either on the efficiencies at several different potentials, or from experiments on the life made at so high a potential-difference that the life will be short, and the experiment made therefore in a comparatively short time.

If, however, an expression of the form

$$f(v) = 10^{a-bv}$$

can be regarded as representing with sufficient accuracy the law of life for all types of incandescent lamps, then if sufficient experiments be made with a number of lamps at each of two different potential-differences to enable us to determine the average life at each of these potential-differences, the constants a and b can be calculated from the equation, and hence the value of $f(v)$, the life, calculated for any other potential-difference.

In connection with this investigation we have endeavoured, with the aid of one of our Assistants, Mr. Walmsley, to ascertain whether some form of ordinary cheap candle could be used, at any rate for rough photometric measurements, in place of the much dearer standard-candles, and, as far as the following results obtained with seven candles selected at random from a packet of No. 8 sperm-candles go, it would seem that these candles do not differ so very much more in intensity from one another than standard candles are said to do. Of course many more experiments on this subject must be made before the possibility of using cheap candles as a rough standard can be decided on, but in the meantime the following experiments may be interesting.

This particular type of candle, No. 8 sperm, and costing 11*d.* per pound, was selected, because such candles were found to resemble in thickness the standard candles that we have been accustomed to use, and which cost 2*s.* 9*d.* per pound.

TABLE IV.

Name of candle.	Grammes of wax burnt per hour.	Light in terms of that emitted by the Standard candle.
Standard candle ...	7.82	1.00
1st Sperm " ...	Doubtful	1.14 (?)
2nd " " ...	6.102	1.00
3rd " " ...	7.188	1.00
4th " " ..	7.29	1.02
5th " " ...	7.1	1.02
6th " " ...	6.84	1.05
7th " " ...	6.66	0.99

These tests of candle-power were not made with any very high degree of accuracy; but the comparison of these No. 8 sperm-candles with the standard was carried out with probably quite as much accuracy as is employed in making ordinary commercial experiments on the luminosity of incandescent lamps.

XXXV. *Intelligence and Miscellaneous Articles.*

ON THE LIMIT OF THE DENSITY AND ON THE ATOMIC VOLUME OF GASES, AND PARTICULARLY OXYGEN AND HYDROGEN. BY E. H. AMAGAT.

THE recent researches on the density of liquid oxygen by MM. Cailletet and Hautefeuille, Pictet, and by Wroblewski, have led to values all of which are less than unity, in the different conditions in which this density has been determined. It has been concluded from this that, in agreement with the previsions of Dumas, it will be equal to unity under a sufficiently powerful pressure, or a sufficiently low temperature, and that accordingly the quotient of the atomic weight by the density, or the atomic volume, would be virtually the same for oxygen, sulphur, selenium, and tellurium.

In my second memoir on the compressibility of gases under strong pressures (*Annales de Chimie et de Physique*, 5th series, vol. xxii. 1881), I showed that at sufficiently high temperatures the law of the compressibility of gases is ultimately represented by straight lines which correspond to the ratio $p=(v-a)=\text{constant}$, which at once gives the limiting volume a for p equal to infinity, and therefore the limiting density; and that for lower pressures the curves, starting from a considerable pressure, exhibit a portion which is virtually rectilinear, and by which we can calculate, though with less certainty, the limiting volume*.

* In this memoir I have used the term *atomic volume* to denote the value of a in reference to one litre of gas at zero, and under a pressure of 76 centims. I make this observation to avoid any confusion.

For oxygen and nitrogen the direction of the rectilinear parts is not yet sufficiently marked, and to obtain it far higher pressures must be obtained than has hitherto been the case. With this view I have constructed apparatus in which I have been able to compress gas to about 4000 atms. Two accidents in succession have hitherto prevented me from making regular numerical determinations; but I may already announce the following result:— I have frequently reduced oxygen to the nine-hundredth of its volume; in these conditions its density was far higher than that of water; with the greatest pressure I could produce, density was higher than 1.25, and this when *the surrounding temperature was 17°*; we must therefore abandon the idea of unity as a limiting density.

Compared with hydrogen, numbers are given as the density of this body in the liquid state which present such extraordinary differences that I shall not attempt to discuss them. The limiting density, as deduced from my own experiments, is very nearly equal to 0.12. Referring to the curve of L. Meyer, it follows that the top of the ordinate which represents the atomic volume of hydrogen will be on the prolongation of the curve, passing through the tops of lithium, of sodium, and of potassium. This would enable us, in conformity with the ideas of Dumas, to assume between this body and lithium a period analogous to those which succeed, but in which, it is true, no known body could be placed.

The two accidents mentioned above appear to me to present sufficient interest to be noticed. In one of the experiments the gas-manometer was contained in a cylinder of cast steel with very thick sides, and with mercury in the bottom. All at once a screeching sound was heard, and a jet of pulverized mercury was shot through the right section of the breach, striking the base of the apparatus, and bounding off to more than 1 metre in all directions, producing the same sound as steam escaping from a high-pressure boiler; the section showed no defect when seen under the microscope. We have therefore before us the classical experiment of the mercurial rain in the form of a true jet of mercury passing through the pores of steel through a thickness of 0.08 metre. The pressure was certainly at least 4000 atmospheres; the same apparatus under the same pressure does not allow a drop of glycerine to pass; this is probably the case also for water and other liquids.

Another apparatus, a solid block of steel with a closer grain, weighing 116 kilog., was split parallel to the axes of the cylinder, and although there was neither projection, nor even separation of the parts any more than a sudden entrance of the gas, the fracture took place with extreme violence; the mercury escaped through the crack, as I had time to observe, in the form of a bright plane vertical metallic sheet of 0.06 to 0.07 metre in breadth.

Among other apparatus which I have used for measuring pressure, I may mention a manometer of Desgoffe's, in which I have introduced the improvement already pointed out by M. Meraud Deprey for piston-manometers. This apparatus should give ex-

cellent results; I do not know within what limits of pressure it can be employed.

The measurement of the volumes of gas has been made by various methods, one of which, based on the use of electrical currents, was communicated to me by Professor Tait of Edinburgh; I shall describe it in my next communication.—*Comptes Rendus*, March 2, 1885.

ON THE ELECTROMOTIVE ACTION OF ILLUMINATED SELENIUM,
DISCOVERED BY MR. FRITTS OF NEW YORK. BY WERNER
SIEMENS.

Mr. Fritts, of New York, sent me last summer a description of his method of preparing selenium plates sensitive to light, which differs from mine in many essential points, and he also sent some of the plates he had prepared. Unlike mine*, they do not consist of parallel platinum wires which are imbedded in a thin layer of selenium, but of a thin homogeneous layer of selenium which is spread on a metal plate, and after being heated, to convert the amorphous into crystalline selenium, is coated with fine gold-leaf. Mr. Fritts found that the green light which has passed through the gold leaf increases the electrical conductivity of the selenium which it traverses. In fact the conductivity of the selenium plate between the gold-leaf and the metal base is increased from 20 to 200 times as much, when the sun's rays fall vertically upon it. And even when illuminated by diffused light, the action is greater with Mr. Fritts's preparation than with my own. One of the plates sent exhibited no sensitiveness to light, but had another and highly remarkable property, namely, that a galvanometer interposed between the gold-leaf and the base plate indicates the existence of an electrical current in the same direction as the motion of the light, as long as the gold-leaf is illuminated. I imagined at first that this current was not permanent, but resembled a polarization-current which only continued until the molecular modification of the selenium by the illumination was complete; and my first experiment appeared to support this assumption. More recent and more thorough experiments have convinced me that this supposition was erroneous. We are here dealing with a phenomenon which is of the greatest scientific importance. My experiments have shown that when the gold-leaf is illuminated, a difference of potential is established, which apparently is proportional to the light and which lasts as long as the illumination. Obscure thermal rays do not act as electromotors, and accordingly the assumption that the action is thermoelectric is excluded. Mr. Fritts assumes that the waves of light which penetrate the selenium are directly converted into electrical currents, and this view is

* *Monatsber. der Berl. Akad. der Wissenschaften*, May 13, 1875, and Jun 7, 1877.

favoured by the proportionality of the strength of the current to the intensity of light. This resulted approximately from the experiments collated in the following table:—

Intensity of light in standard candles ...	}	6.4	9.9	12.8	16.8
Strength of current ...		18	30	40	48
Quotient		2.8	3	3.1	2.8

The luminosity was determined by means of a Bunsen's photometer, and the strength of the current was determined by the deflection of a delicate reflecting-galvanometer.

When the gold-leaf was exposed to illumination from the south-east side of the unclouded sky, while the sun itself was hidden by adjacent high buildings, the measurements given in the following table were obtained.

	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	
Time of ob- servation...	9 37	10 5	10 30	11 0	11 35	12	12 30	1 0	1 30	2 0	2 30	3 0	
Deflection of the galvanometer.	}	190	196	209	223	250	250	244	245	249	228	188	173
Time		3.30	4										
Deflection ...		172	108										

It follows from this that the electromotive force of the selenium plate increased pretty regularly from the morning at 9.30 to mid-day at 11.35, and then remained constant with some variations, after which it decreased pretty regularly to 3 o'clock.

Mr. Fritts is unable to offer any explanation as to the reason why some plates become better conductors, and some act as electromotors; he complains of the uncertainty in the preparation of the selenium plates, the properties of which cannot be foreseen, and he gives various methods of dealing with them, by which plates which are often inactive may be used. More thorough investigations will therefore be necessary to establish what is due to the electromotive action of light on many selenium plates. Nevertheless the existence of a single selenium plate with the properties described is a fact of the greatest scientific importance, for here we meet for the first time with an instance of the direct conversion of the energy of light into electrical energy.—*Sitzungsberichte der Akad. der Wissen. zu Berlin*, Feb. 12, 1885.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MAY 1885.

XXXVI. *Notes on the Use of Nicol's Prism.* By JAMES C. M'CONNEL, B.A., Assistant Demonstrator at the Cavendish Laboratory, Cambridge*.

1. *On the Error in the Measurement of a Rotation of the Plane of Polarization caused by the Axis, about which the Nicol turns, not being parallel to the Incident Light.*

SUPPOSE we have a beam of parallel light traversing a Nicol's prism mounted in a graduated circle. Unless we have taken special precautions, we shall find that, when the Nicol is rotated, the plane of polarization of the emergent light turns through an angle somewhat different from that measured by the circle. For instance, if the axis of rotation of the Nicol is inclined 3° to the direction of the incident or emergent light, as large an error as 1° may be made in measuring a rotation of 60° .

It is, however, tolerably well known that, as far as this error is proportional to the first power of the small angle of deviation, it may be eliminated by taking the mean of the readings in the two opposite positions of the Nicol circle separated from one another by nearly two right angles. It is of course only to a first approximation that this proportionality can be considered to hold good. And the main object of the present investigation is to determine, what is the outstanding error we are liable to in assuming this proportionality; or, in other words, with what accuracy we must adjust the Nicol circle, that this first approximation may be sufficient.

* Communicated by the Physical Society. Read February 28, 1885.
Phil. Mag. S. 5. Vol. 19. No. 120. May 1885. Z

The phenomenon in its simplest form may be described thus:—If the emergent ray be parallel to the long edges of the prism it is polarized in a plane perpendicular to the principal plane of the prism, *i. e.* to the plane containing the optic axis and the normal to the face. If the ray be inclined to this position but still lie in the principal plane, then the new plane of polarization is as nearly parallel to the old one as is consistent with its containing the new ray. This indeed is obvious from symmetry. But now let the ray lean out of the principal plane: there is a marked change in the plane of polarization. It has been twisted about the ray, and the angle of twist is nearly one third of the angle of deviation. From this explanation it is easy to see in a general way how the error arises, and also how it may be eliminated by taking readings in the two opposite positions of the Nicol.

There is another important point that is apparent on the face of the matter. If the direction of the ray coincide with the axis of rotation, the ray will not move relatively to the prism when the prism is turned, and the rotation measured by the circle will be identical with the actual rotation of the plane of polarization. We shall find, moreover, that to the second order of approximation the final error depends entirely on the angle between these two directions (see equation 8). It is independent of any reasonably small errors in the adjustment of the Nicol in the circle.

In a Nicol's prism used as a polarizer, it is the second half of the prism that determines the plane of polarization of the emergent light. If the optic axis of the spar in the first half be not accurately parallel to the axis in the second half, the effect is not to turn the plane of polarization, but merely to mix a little ordinary light with the emergent polarized light.

The figure represents a portion of the sphere of unit radius.

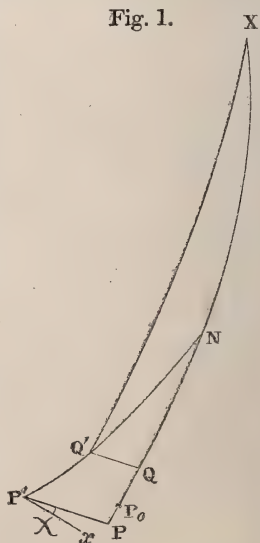
X is the optic axis. N the normal to the face.

So XN is the principal plane.

P' is the wave-normal of the emergent light.

Q' is the wave-normal of the internally incident light.

Draw Q'Q, P'P perpendicular to XN produced. Join XQ'.



Let $P'x$ be the plane of polarization of P' and $PP'x = \chi$. Then χ is a function of $P'P$ and PN .

The exact function would be a very complicated expression, so we shall content ourselves with an approximation. $P'P$ is always small, and P lies near some fixed point, P_0 say, in the plane XN . Let

$$P'P = \alpha, \quad PP_0 = \beta.$$

We shall reject the cubes and higher powers of α and β and limit ourselves to expanding χ as far as the squares. We know that when α is zero, χ is zero; so our expression can only contain the terms α , α^2 , and $\alpha\beta$.

In a Nicol's prism it is the extraordinary ray which emerges, so the plane of polarization of Q' is perpendicular to $Q'X$. Let us assume for the present that the plane of polarization of P' makes the same angle with the plane of incidence $P'Q'N$ as does the plane of polarization of Q' . The plane of polarization of Q' makes with $P'Q'N$ an angle $90^\circ - NQ'X$. Hence

$$\chi = 90^\circ - NQ'X - NP'P. \quad \dots \quad (1)$$

But in the triangle $NP'P$, P is a right angle; so we have

$$\tan(90^\circ - NP'P) = \cot NP'P = \frac{\sin P'P}{\tan NP} = \frac{\alpha}{\tan NP}, \quad \dots \quad (2)$$

neglecting cubes.

It remains to find $NQ'X$.

$$NQ'X = XQ'Q - NQ'Q,$$

$$\therefore \chi = 90^\circ - NP'P + NQ'Q - XQ'Q. \quad \dots \quad (3)$$

By spherical triangles,

$$\left. \begin{aligned} \tan(90^\circ - XQ'Q) &= \frac{\sin Q'Q}{\tan XQ}, \\ \tan(90^\circ - NQ'Q) &= \frac{\sin Q'Q}{\tan NQ}. \end{aligned} \right\}$$

Also

$$\left. \begin{aligned} \sin Q'Q &= \sin Q'N \sin N, \\ \sin P'P &= \sin P'N \sin N. \end{aligned} \right\}$$

But $\sin P'N = \mu \sin Q'N$, where μ is the extraordinary index of refraction for that wave, which we may take to be constant.

$$\therefore \sin Q'Q = \frac{1}{\mu} \sin P'P = \frac{\alpha}{\mu} \text{ to our order of approximation.}$$

Hence

$$\left. \begin{aligned} \tan(90^\circ - XQ'Q) &= \frac{\alpha}{\mu} \frac{1}{\tan XQ}, \\ \tan(90^\circ - NQ'Q) &= \frac{\alpha}{\mu} \frac{1}{\tan NQ}. \end{aligned} \right\} \dots \quad (4)$$

Since $\tan XQ$ and $\tan NQ$ occur in small terms, we may reject squares in finding their values.

In NQ let us take a point Q_0 such that $\sin NP_0 = \mu \sin NQ_0$. Then, since $\sin (NP_0 + \beta) = \mu \sin (NQ_0 + QQ_0)$,

$$\beta \cos NP_0 = \mu QQ_0 \cos NQ_0,$$

and

$$\left. \begin{aligned} \frac{1}{\tan XQ} &= \frac{1}{\tan XQ_0} - \frac{QQ_0}{\sin^2 XQ_0} = \frac{1}{\tan XQ_0} - \frac{\beta \cos NP_0}{\mu \cos NQ_0 \sin^2 XQ_0}, \\ \frac{1}{\tan NQ} &= \frac{1}{\tan NQ_0} - \frac{QQ_0}{\sin^2 NQ_0} = \frac{1}{\tan NQ_0} - \frac{\beta \cos NP_0}{\mu \cos NQ_0 \sin^2 NQ_0}. \end{aligned} \right\} (5)$$

Now in the expansion of the tangent of a small angle the squares of the angle do not appear. So we have by (3), (2), and (4),

$$\chi = \frac{\alpha}{\tan NP} + \frac{\alpha}{\mu} \frac{1}{\tan XQ} - \frac{\alpha}{\mu} \frac{1}{\tan NQ},$$

while

$$\frac{1}{\tan NP} = \frac{1}{\tan NP_0} - \frac{\beta}{\sin^2 NP_0}.$$

So by (5),

$$\begin{aligned} \chi &= \alpha \left\{ \frac{1}{\tan NP_0} + \frac{1}{\mu \tan XQ_0} - \frac{1}{\mu \tan NQ_0} \right\} \\ &+ \alpha \beta \left\{ -\frac{1}{\sin^2 NP_0} - \frac{\cos NP_0}{\mu^2 \cos NQ_0} \left(\frac{1}{\sin^2 XQ_0} - \frac{1}{\sin^2 NQ_0} \right) \right\} (6) \\ &= l\alpha - m\alpha\beta \text{ say.} \end{aligned}$$

In Nicol's prism, as it is usually cut,

$$NX = 41\frac{1}{2}^\circ,$$

$$NP_0 = 22^\circ,$$

$$\mu = 1.54,$$

$$\therefore NQ_0 = 14^\circ.$$

Substituting these values, we obtain

$$l = .32, \quad m = .84;$$

$$\chi = .32\alpha - .84\alpha\beta. \quad . \quad . \quad . \quad . \quad (7)$$

In the foregoing we have assumed that there is no rotation of the plane of polarization on refraction out of the spar, or, in other words, that the angle between the plane of polarization and the plane of incidence remains unchanged. There is of course a change, but it is merely due to the disturbing effect of reflection and is very small. If we treat the spar as

a homogeneous medium and use Fresnel's formulæ, we find the rotation is about $\cdot 005 \alpha$ in the case of an ordinary Nicol.

I have calculated from Neumann's theoretical formulæ for refraction at the surface of crystals what the rotation would be in the case of light entering the Nicol and exciting only the extraordinary wave, and find it is less than with a homogeneous medium. In the case of light leaving the Nicol there are two reflected waves, so the formulæ would probably be very complicated. I think we may safely assume that the rotation is less than $\cdot 01 \alpha$, and may therefore be neglected. In the case of a flat-ended Nicol the incidence is nearly direct, and the rotation may obviously be neglected.

Owing to the circumstance that this rotation is negligible, the whole of this investigation applies without alteration to the case when the Nicol is used as an analyzer, and the light is consequently incident on the spar. In this case we have to find the position of the plane of polarization that only the ordinary wave may be excited, and the formula for the rotation is the same as in an isotropic medium.

We are now in possession of a convenient formula for expressing the position of the plane of polarization in terms of the direction of the emergent light relative to fixed planes in the Nicol. We shall apply this formula to the discussion of the error in measuring a rotation. This part of the investigation would be very short if we confined ourselves to the first approximation. A number of complications are introduced by the necessity of retaining the squares and products of small quantities.

The figure represents a portion of the sphere of unit radius. Let

A be the axis of rotation,

NP the principal plane,

P' the emergent wave-normal,

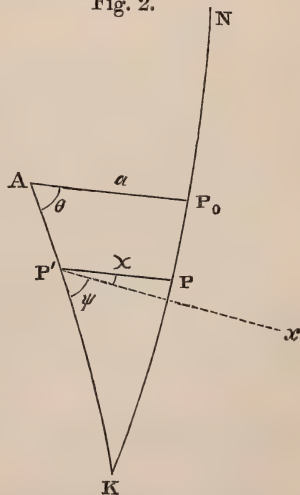
P'x the plane of polarization.

Draw AP_0 and $P'P$ perpendicular to the principal plane. This fixes P_0 , which has hitherto been arbitrary within certain limits. A and P' are fixed in space, while N and P_0 are rotated round A.

For simplicity, suppose the circle-reading is zero when P' lies on AP_0 . Then $\theta = \angle P_0AP'$ is the reading at any time.

Produce AP' to meet NP_0 in K.

Fig. 2.



What we want to measure is the rotation of $P'x$ round P' ; so let $xP'K = \psi$. As before, let $P'P = \alpha$, $PP_0 = \beta$, and let $AP_0 = a$, $AP' = r$. Then α , β , a , r are all small. Since the figure $AP_0P'P$ is small, we have to our order of approximation

$$\left. \begin{aligned} \alpha &= a - r \cos(\psi + \chi), \\ \beta &= r \sin(\psi + \chi). \end{aligned} \right\}$$

Let us now find the difference between θ and $\psi + \chi = PP'K$. By spherical triangles,

$$\left. \begin{aligned} \cos K &= \sin(\psi + \chi) \cos PP', \\ \cos K &= \sin \theta \cos a; \end{aligned} \right\}$$

$$\therefore \sin(\psi + \chi) \cos PP' = \sin \theta \cos a,$$

$$\sin \theta = \sin(\psi + \chi) \left(1 - \frac{\alpha^2}{2} + \frac{a^2}{2}\right),$$

$$\sin(\psi + \chi) + (\theta - \psi - \chi) \cos(\psi + \chi) = \sin(\psi + \chi) \left(1 - \frac{\alpha^2}{2} + \frac{a^2}{2}\right);$$

$$\therefore \theta - \psi - \chi = \frac{a^2 - \alpha^2}{2} \tan(\psi + \chi)$$

$$= \frac{a^2 - \alpha^2}{2} \tan \psi, \text{ neglecting cubes.}$$

But

$$a^2 - \alpha^2 = 2ar \cos \psi - r^2 \cos^2 \psi,$$

and

$$\chi = l\alpha - m\alpha\beta$$

$$= l(a - r \cos \overline{\psi + \chi}) - mr \sin \psi (a - r \cos \psi)$$

$$= l\{a - r \cos \psi + lr \sin \psi (a - r \cos \psi)\} - mr \sin \psi (a - r \cos \psi)$$

$$= la - lr \cos \psi + (l^2 - m)r \sin \psi (a - r \cos \psi);$$

$$\therefore \theta = \psi + la - lr \cos \psi + (l^2 - m + 1)ar \sin \psi - (l^2 - m + \frac{1}{2})r^2 \sin \psi \cos \psi.$$

Now let the plane of polarization be turned through 180° and the new reading of the Nicol circle be $180^\circ + \theta_1$. Instead of ψ we must write in the last formula $180 + \psi$. So

$$\theta_1 = \psi + la + lr \cos \psi - (l^2 - m + 1)ar \sin \psi - (l^2 - m + \frac{1}{2})r^2 \sin \psi \cos \psi;$$

$$\therefore \frac{\theta + \theta_1}{2} = \psi + la - (l^2 - m + \frac{1}{2})r^2 \sin \psi \cos \psi,$$

$$\frac{\theta_1 - \theta}{2} = lr \cos \psi - (l^2 - m + 1)ar \sin \psi.$$

Inserting numerical values, we have

$$\frac{\theta + \theta_1}{2} - \psi = .32a + .24r^2 \sin \psi \cos \psi, \quad . \quad . \quad . \quad (8)$$

$$\theta_1 - \theta = .64r \cos \psi, \text{ approximately. } . \quad . \quad . \quad . \quad (9)$$

To render these formulæ intelligible to one who has not read through the investigation, we may remark that we have to deal with one plane fixed in space, viz. the plane containing the axis of rotation of the Nicol circle and the direction of the emergent light.

θ and $180^\circ + \theta_1$ are the readings of the circle in the two opposite positions, and $\theta = 0$ when the principal plane of the Nicol is at right angles to the fixed plane.

ψ is the angle between the plane of polarization and the fixed plane.

a is the angle between the axis of rotation and the principal plane.

r is the angle between the axis of rotation and the emergent light.

The angles are supposed to be expressed in circular measure in these as in all the other formulæ.

The first term on the right-hand side of equation (8) is a constant, and is therefore of no consequence in measuring a change of ψ . The second term is a measure of the outstanding error.

Let us suppose

$$r = 3^\circ, \quad \psi = 30^\circ.$$

Then

$$.24r^2 \sin \psi \cos \psi = 1';$$

but if $\psi = -30^\circ$,

$$\text{the same} = -1'.$$

So in measuring a rotation of 60° we may be subject to an error of $2'$ if the axis of rotation be inclined to the emergent light at an angle of 3° . To make sure that the error shall be less than $1'$, the last-mentioned angle must be kept within 2° .

Equation (9) affords the means of deducing the value of r from the difference of readings in the two opposite positions.

The second term of (8) is due to two main causes. One is that the rotation of the plane of polarization takes place about one axis, while the rotation is measured about another. The effect of this appears in the coefficient $-\frac{1}{2}$. The other part of the error is due to the values of χ in the two opposite positions not exactly neutralizing each other. This appears in the coefficient $m - l^2$. If these two causes had reinforced instead of counteracting one another, the resultant error would have been five times as large.

If the arrangements permit of taking readings when the plane of polarization is inclined at 90° to its two first positions, we can, by taking the mean of all four readings, get rid of the error depending on the squares of small quantities, as is evident from (8). This is what Lord Rayleigh has done in his recent measurements of the electromagnetic rotation in bisulphide of carbon.

The formula (6) applies to almost every mode of cutting a Nicol, except the important case when the ends are cut off square to the length, the case of the so-called "flat-ended Nicol." This case requires the investigation to be modified. The peculiarity consists in N lying very close to P'.

$$\chi = 90^\circ - NP'P + NQ'Q - XQ'Q,$$

as before ;

and by (4) and (5),

$$\tan (90^\circ - XQ'Q) = \frac{\alpha}{\mu \tan XQ_0} - \frac{\alpha\beta}{\mu^2 \sin^2 XQ_0}.$$

If we take N as the point from which to measure β , P₀ and Q₀ will coincide with N.

We have now to find $NP'P - NQ'Q$. The process is precisely similar to one already performed in finding equation (8); and we obtain

$$\begin{aligned} NP'P - NQ'Q &= \frac{PP'^2 - QQ'^2}{2} \tan NP'P \\ &= \frac{\mu^2 - 1}{2\mu^2} \alpha\beta; \end{aligned}$$

$$\begin{aligned} \therefore \chi &= \frac{\alpha}{\mu \tan XN} - \frac{\alpha\beta}{\mu^2 \sin^2 XN} - \frac{\mu^2 - 1}{2\mu^2} \alpha\beta \\ &= l\alpha - m\alpha\beta, \text{ say.} \end{aligned}$$

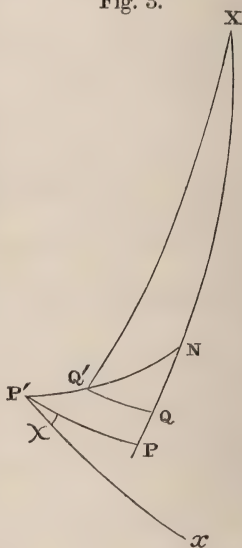
Taking $XN = 63^\circ$, $\mu = 1.52$, we obtain

$$l = .33, \quad m = .83.$$

So the numbers are almost exactly the same as in the case of the ordinary Nicol.

As a test of the accuracy of the above calculations, I made a few observations on a flat-ended Nicol. I found that, turning the Nicol through $30'$, about an axis in the principal plane

Fig. 3.



and at right angles to the incident light, turned the plane of polarization through $11' \pm 1'$.

In a paper on Polarizing Prisms (Phil. Mag. 1883) Mr. Glazebrook has suggested a new form of flat-ended prism in which the axis of the spar is at right angles to the length of the prism. In this case $XN = 90^\circ$, and $\mu = 1.49$.

$$\therefore l=0, \quad m=.73,$$

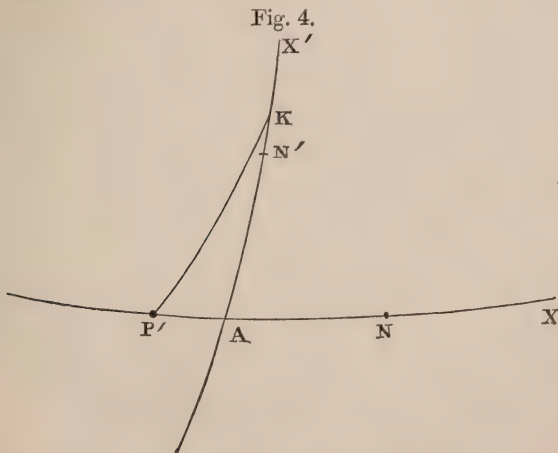
$$m-l^2-\frac{1}{2}=.23.$$

So instead of equation (8) we have

$$\frac{\theta + \theta_1}{2} - \psi = .23r^2 \sin \psi \cos \psi,$$

and the outstanding error is substantially the same as before.

Equation (9) appears to be inconsistent with a result obtained by Mr. Glazebrook (Phil. Mag. Oct. 1880, p. 252). For the sake of simplicity, he supposed the ordinary ray only to emerge from the crystal. He examined two positions of the Nicol. First the emergent ray lay in the principal plane, so that the plane of polarization coincided with the principal plane. Next he supposed the Nicol to be turned through 90° about an axis, lying in the principal plane, but inclined at 5° to the emergent ray, and he found that now the plane of polarization was inclined at $5^\circ 3'$ to the principal plane. *But this does not show that the plane of polarization has been turned through $90^\circ \pm 5^\circ 3'$ about the direction of the ray, as is evident on examination of the annexed figure.*



P' is, as before, the emergent light ; N, N', X, X' are the two positions of the normal to the face and the axis.

In the first position the plane of polarization is $P' A N X$.

In the second position let it be $P' K$.

Mr. Glazebrook finds $P' K A = 5^\circ 3'$.

The angle, however, that we wish to find, is the angle through which the plane of polarization has been turned about the direction of the ray, viz. $A P' K$, and this, of course, is not equal to $90^\circ - P' K A$.

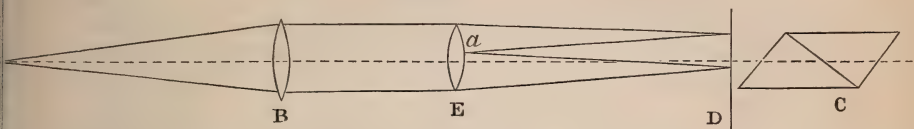
After I had written the greater part of this paper, I found that the ground had been already traversed by Sande Bakhuyzen (*Pogg. Ann.* cxlv. p. 259, 1872). His investigation is considerably longer than mine, and he only examines the case of the ordinary Nicol. He obtains results of the same general form; but he falls into at least one serious error; so his numerical values are quite different. Instead of assuming, as I have done, that the rotation of the plane of polarization on refraction out of the crystal is negligible, he uses a complicated formula, giving directly the position of the plane of polarization of the incident ray in which only the extraordinary ray is excited in the crystal. This formula he has apparently deduced from a formula given by Neumann (*Pogg. Ann.* xlii. p. 11). And it is here that the error occurs. He has not paid sufficient attention to Neumann's explanation of how the quantities in the formula are to be measured. He puts β for ω ; whereas he should put $\beta - \pi$ for ω . Making this correction in his equation (5), we find all the terms in the denominator are of one sign, and the value of $\tan A$ is considerably diminished. We find, too, that $90^\circ - A$ is practically the same as the angle between the plane of incidence and the plane of polarization in the crystal, which is what I have assumed.

In this very same operation Bakhuyzen falls into another mistake. He is considering the case of light emerging from the Nicol, while he uses the formula for light entering the crystal. Even with glass this would have been wrong; but with crystal the cases are totally distinct, for when light is refracted out of the crystal there are two reflected rays instead of only one.

It may not be out of place here to describe a simple method for testing the necessary adjustment, which I lately found convenient. In my arrangement (fig. 5) the source of light was an illuminated slit A , whence the light passed through a lens B , placed so that the slit was at its principal focus. Thus a parallel beam of light fell on the polarizing Nicol C . To the Nicol circle I attached a plane mirror D as nearly as possible at right angles to the axis of rotation. Between B and D , I interposed a lens E at a distance of half its focal length from D . An image of the slit is thus formed at a on the surface

of the lens E, or rather on a scrap of paper attached to it. The image *a* of course remains at the same point on the paper,

Fig. 5.



however the lens E is moved laterally in its own plane. On rotating the Nicol circle I found that *a* did not move ; so I knew that the mirror was at right angles to the axis of rotation. I then adjusted the Nicol circle till the middle point of the image of the slit fell on the optical centre of the lens, which I had previously marked on the paper. If I had chosen a wrong point as the optical centre, the error would have been at once evident on turning the lens E through 180° in its own plane. This method gave without much difficulty an accuracy of 1° , with a lens E of only 4 inches focal length.

2. On a new Method of obtaining the Zero-reading of a Nicol Circle.

In a large class of experiments on polarized light, it is necessary to know the reading of the Nicol circle when the plane of polarization is parallel to the axis of rotation of some part of the apparatus, *e. g.* of the table of a spectrometer. The usual method of obtaining this reading depends on the polarizing power of a glass reflecting surface. It is easy to fix the surface on the spectrometer-table parallel to the axis of rotation. If the angle of incidence be made that of maximum polarization, and the Nicol turned till the reflected light is reduced to a minimum, the plane of polarization is then perpendicular to the plane of incidence. This method is simple, but it is not very sensitive. Even with poor illumination the minimum reflected light is, with glass at any rate, by no means evanescent ; and as the Nicol is turned the intensity remains sensibly constant for some distance on either side of the minimum point. The sensitiveness, too, does not increase, but rather falls off with a more powerful light.

The method I am about to describe is nearly as simple ; and, as it depends on the crossing of two Nicols, its sensitiveness is only limited by the power of the source of light. In its simplest form the process is as follows :—An auxiliary Nicol is fixed on the table of the spectrometer, the polarizer turned till the light is quenched, and the reading taken.

Then the table of the spectrometer is turned through two right angles—the axis of rotation having been previously set perpendicular to the incident light—the light quenched, and the reading taken again. *The plane of polarization now leans as much to one side of the axis as it did before to the other.* So the mean of the two readings is the reading when the plane of polarization is parallel to the axis of rotation of the table.

The above is of course only a general explanation. The statement in italics requires fuller examination, and we shall find that it is only strictly true when the Nicol is symmetrical. Let us, then, first suppose that the Nicol is perfectly symmetrical—that is, that the two faces are parallel and the optic axes of the two halves coincident. We shall assume throughout the investigation that the axis of rotation is accurately perpendicular to the incident light. In the previous note we have carefully determined the position of the plane of polarization in terms of the direction of the emergent light, and we have shown that, to a very close approximation, it is at right angles to that position of the plane of polarization of light incident along the same path which gives complete extinction in the Nicol. The discrepancy was shown to be negligible. So there is no ambiguity in speaking of the plane of polarization of either half of the Nicol for a particular direction of the light.

In the first position of our Nicol on the spectrometer, the emergent light is parallel to the incident, and the planes of polarization of the two halves are also parallel. The axis of rotation, too, is at right angles to the incident light. Imagine, then, a line fixed relatively to the Nicol to be drawn from the second face parallel to the emergent light. When the Nicol has been rotated through two right angles, this line becomes again parallel to the incident light, and the plane of polarization of the second half of the Nicol now leans exactly as much to one side of the axis of rotation as the plane of polarization of the first half leant before to the other. The process therefore is so far strictly accurate.

Secondly, let us suppose that the Nicol, though not symmetrical, yet produces no angular deviation on light traversing it. We have seen in the previous note that the plane of polarization of a Nicol is nearly at right angles to the principal plane, and that to a first approximation the small deviation is proportional to the angle between the external light and the principal plane.

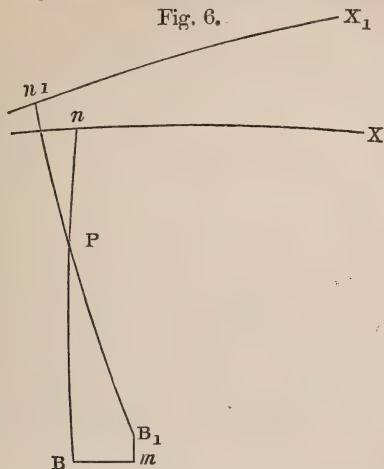
Let X_n , X_1n_1 be the principal planes of the two halves.

B , B_1 their poles.

P the direction of incident and emergent light.

Draw Bm at right angles to BP and B_1m at right angles to Bm . We may assume that BB_1 is very small and BP

Fig. 6.

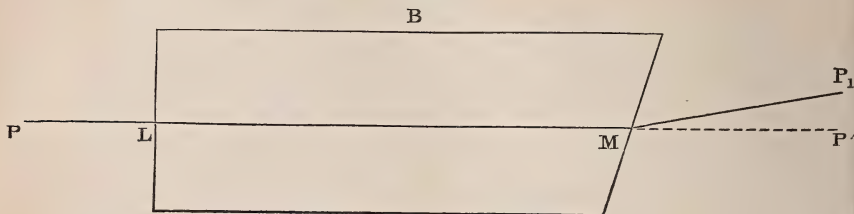


nearly a right angle. The angle between the planes of polarization of the two halves is composed of two parts, of which one, BPB_1 , is approximately equal to Bm , and the other is approximately proportional to the difference between Pn_1 and Pn and therefore to B_1m . So the angle is independent of small variations in the position of P , and is therefore a constant for each particular Nicol. In some Nicols, as we go from one half to the other, the plane of polarization is turned in the direction of a right-handed screw. These we may call right-handed Nicols with a certain rotation-angle. Supposing, then, our auxiliary Nicol is right-handed, the plane of the second half leans at first too much to the right, but, when the Nicol has been rotated through 180° , it leans too much to the left. The correct reading in the case of a right-handed Nicol is therefore that obtained by turning the Nicol circle from the mean reading through half the rotation-angle in the right-handed direction.

The question now arises, how we are to determine the rotation-angle. The following process explains itself. Place the auxiliary Nicol B on the path of the light from the polarizer A , which is mounted in a graduated circle. Turn A till the light is quenched and take a reading. Then turn A so as to allow the light to pass through B , and place in the path of the light from B a third Nicol C . Adjust C till the light from B is quenched. Remove B , cross C with A , and read again.

So far the matter is tolerably simple, but if the Nicol produce deviation of the light complications are introduced.

Fig. 7.



The figure represents a flat-ended Nicol B traversed by a ray of light $PLMP_1$. The argument, however, is perfectly general. A small error is due to the Nicol C having to deal first with light parallel to MP_1 and afterwards with light parallel to PP' . This may be eliminated by taking two readings, the Nicol B in the interval having been turned through two right angles or thereabouts about LM. The mean thus found gives us the position of the plane of polarization of PL, which is most nearly parallel to the plane of polarization of MP_1 . We know nothing about the plane of polarization of light which issues parallel to MP' . Clearly therefore, when our Nicol is mounted on the spectrometer-table, we ought not to turn it exactly through 180° , but we ought to turn it till the incident light is related to the face M in the same way as P_1M was at first.

We have then this practical rule. Place the auxiliary Nicol on the spectrometer in such a position that the deviation lies wholly in the plane of rotation. To get the second reading, turn the table in the direction of the deviation through an angle equal to 180° less the deviation. Correct the mean of the two readings by half the rotation-angle as before.

Thus we are able to get an accurate value of the zero-reading in spite of the dissymmetry of the Nicol and the deviation of the light, provided only the two faces of the Nicol are good planes. We have been compelled, however, to limit ourselves to first approximations. On this point it is to be noticed that the outstanding error is due to two causes, the inaccuracy of adjustment of the auxiliary Nicol, and the dissymmetry of the Nicol itself. The latter we may assume to be very small; while the former has very slight effect. It only comes in as a secondary cause, for if the dissymmetry were removed the outstanding error would be zero. Hence it is not necessary in general to take elaborate precautions about the setting of the Nicol in the determination either of the zero-reading or of the rotation-angle. If great accuracy be

required, it will be sufficient to attach permanently to the Nicol a small reflecting surface, and make this always perpendicular to the incident light. The planes of polarization of both halves will then be perfectly definite.

I have treated the method as applied to a Nicol used as polarizer, but it is by no means restricted to this case. The polarizer may be any kind of polarimeter, or if necessary the polarimeter may be the analyzer, while the auxiliary Nicol is used as polarizer. The method occurred to me when I was engaged on some observations on the refraction of polarized light at the surface of Iceland spar. I will describe the arrangements in so far as they bear on the matter in hand; for, as it happened, they gave a very high degree of accuracy in the determination of the zero-reading.

As my source of light I employed part of the filament of a Swan incandescent lamp. I selected a straight piece, and cut off the light from the rest of the filament with a diaphragm placed immediately in front of the lamp. The filament was placed at the principal focus of a lens, from which the light passed to the polarizing Nicol. Then came the spectrometer, from which the collimator had been dismantled; so only the telescope remained. When the telescope was focussed for infinity and directed towards the light, a sharply defined image of the selected portion of the filament was seen. The top and bottom limits of the image were not hard lines, owing to the diaphragm not being quite at the principal focus of the collimating lens. They were, however, sufficiently definite for practical purposes.

It was necessary for my other observations that the axis of rotation of the spectrometer should be set at right angles to the light coming from the middle of the filament. I accomplished this in the following manner. I first mounted a reflecting surface on the table of the spectrometer, and adjusted it to be parallel to the axis of rotation. This may be done with accuracy although the telescope be tilted. Next I adjusted the axis of the telescope to be at right angles to this surface, by getting the cross wires to coincide with their image formed by reflection at the surface. Then I removed the reflecting surface, directed the telescope towards the light, and tilted the spectrometer till the cross wires coincided with the middle of the image.

When the auxiliary Nicol was mounted on the spectrometer-table and the polarizer turned past the crossed position, the image of the filament by no means entirely disappeared; but a patch of nearly complete extinction moved down the image from the top to the bottom. This was of course to be expected, since different positions of the image correspond to different

directions through the Nicols, and therefore to different positions of the two planes of polarization. The reading was taken when the patch was halfway down the slit. This method of reading proved to be very sensitive. Without any special care I could get a number of successive readings whose greatest difference was $1'$. This was quite sufficient for my purposes. Indeed my verniers were only graduated to $1'$. But I am convinced that a far higher degree of sensitivity could be reached if desired, by proceeding on the same lines.

In securing great accuracy two precautions become very important. The source of light should be of uniform brightness. Here a well-made carbon filament is nearly perfect. Again, as much light should reach the eye from the top of the filament as from the bottom, abstraction being made of course of what is stopped by the polarizing properties of the Nicols. For this it is convenient that the lateral throw of the Nicols should be as small as possible. Flat-ended Nicols are therefore to be preferred. But if a pair of Nicols were made for the purpose, it would be easy so to slant the faces that the extraordinary ray should go straight through, and there should be no lateral throw.

The incandescent lamp is a more powerful source of light for this purpose than is perhaps at first sight apparent. For it may be shown that with a given slit and object-glass the intensity of the illumination of the surface of the first Nicol depends solely on the intrinsic brightness of the source of light. The filament is practically a slit with the source of light brought into contact with it, so there is no difficulty about the whole of the surface of the Nicol being illuminated, and the illumination is just as powerful as would be given by a glowing sheet of the same brightness half an inch broad placed a little distance behind a slit. The intrinsic brightness of the filament of a Swan lamp is many times greater than that of a good gas-flame viewed edgewise. Besides, even if the gas-flame be placed very close to the slit, it is only the nearer portion of the flame that illuminates the whole surface of the Nicol. The further portion only lights up a narrow strip in the middle of the surface.

I used two flat-ended Nicols, belonging to Mr. Glazebrook, of about half an inch aperture. One of these, which gave an angular throw of $15'$, had a left-handed rotation-angle of $38'$.

I find that the plan of reading by means of the motion of the patch of extinction has been the subject of an elaborate paper by Lippich (*Sitzb. der kais. Akad. der Wissensch. Wien*, Febr. 1882).

XXXVII. *Electromagnets*.—III. *Iron and Steel*. *New Theory of Magnetism*. By R. H. M. BOSANQUET, *St. John's College, Oxford*.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN the February number of the *Philosophical Magazine* (p. 73) I gave a number of experiments on the Permeability (μ) of Iron and Steel, with empirical formulæ founded on Fourier's Series. I also gave the elements of a new theory leading to equations of the form

$$\mu = A(\mathfrak{B}_{\infty} - \mathfrak{B}) \cos \delta,$$

$$\delta = f\theta,$$

$$\mathfrak{B} = \frac{k}{\rho} \frac{60^\circ - \omega\theta}{\sin \theta},$$

and gave the comparison of this theory with experiment in the cases of Crown (soft bar) Iron ring E, and Steel ring J (first soft state). I propose now to communicate the comparisons of all the remaining experiments of the former paper, and of three additional sets on J.

For clearness I will recapitulate shortly the bearing of the formulæ.

Each molecule has one, and only one, axis of transmission (like* a bead with a hole in it). The axis is capable of transmitting \mathfrak{B}_{∞} lines of force and no more, and the molecular permeability is proportional to $\mathfrak{B}_{\infty} - \mathfrak{B}$, or to the defect of saturation. (If we pack the hole in the bead with thin wires, the aperture remaining is represented by the number of wires that remain to be got in.)

A is the molecular permeability per unit defect of saturation.

δ is an auxiliary angle representing the obliquity of a zigzag which would augment the resistance to the amount actually observed.

θ is the average inclination of the axes of the molecules to the direction of magnetization; initially it is always 60° . ω is the unit angle.

f is the factor connecting θ and δ .

$\frac{k}{\rho}$ represents the force of molecular torsion which is in equilibrium with the tendency of the lines of force further to deflect the average molecule.

* These statements are not to be taken as actual hypotheses, but as geometrical analogies of the distribution.

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The inferences as to these constants to be drawn from the numerous experiments now discussed are not so clear as they appeared to be from the two cases treated in the last paper.

The first of the following Tables contains the constants from all the experiments that have been reduced, finishing with those obtained from Rowland's Table I.

The remaining Tables contain the detailed comparison of the theory with experiment in all the cases, except ring E, and J first soft state, which two were given in the last paper.

The last column but one of the first Table gives the product of *A*, the reciprocal of $\frac{\kappa}{\rho}$, and a constant. This quantity may be compared with the maximum observed value of μ in the last column.

A has been spoken of as the molecular permeability for a certain unit. Regarding magnetism as a motion or displacement, whether dynamic or static, we may thus speak of *A* as a coefficient of freedom within the molecule.

$\frac{\kappa}{\rho}$ being the coefficient of the forces which tend to prevent the rotation of the molecule as a whole, we may speak of its reciprocal as a coefficient of freedom without the molecule.

The last column but one may be therefore regarded as the product of coefficients of intra-molecular and extra-molecular freedom, and a constant.

This product (*c* × intra- × extra-molecular freedom) is a characteristic of a given approximate state of a given piece of metal. It runs generally with the maximum of μ ; but is liable to be depressed with respect to μ where *f* has high values, as in the first, second, and fourth entries. As between the hard steel and the iron, the product of the coefficients of freedom is proportional to the maximum permeability.

The further general conclusions, so far as these data go, are as follows:—

In soft steel the molecular forces are chiefly extra-molecular, the freedom intra-molecular.

In hard steel the molecular forces are chiefly intra-molecular, the freedom extra-molecular.

In soft iron the average intra-molecular freedom is much greater than in hard steel, the extra-molecular freedom about the same.

In soft steel the extra-molecular freedom is much diminished, the intra-molecular freedom is moderate or high.

The distribution of the freedom between intra- and extra-molecular conditions depends minutely on the condition of the piece of iron or steel.

The variations thus arising are comparatively moderate in iron, considerable in hard steel, and enormous in soft steel.

In one or two cases the equations do not represent the values quite so well; in G two sets of constants are given.

The dimensions of the rings and details of the experiments are given in my paper of February last.

Steel ring J was examined first soft, then hard, then soft again, then hard again. Its saturation-point is taken to be 20,000 throughout. It was wound with about 4000 turns in the hard states.

P.S.—From the above conclusions we can deduce the outline of a chemical theory of the hardening and tempering of steel.

In hard steel the additional constraint, as compared with iron, is wholly intra-molecular, *i. e.* due to chemical combination with a steeling element. This combination must exist naturally at a red heat, and be stereotyped by sudden cooling.

In soft steel the intra-molecular constraint is much diminished, and the extra-molecular constraint increased. Hence in slow cooling dissociation of the steeling element takes place. This requires time. At any point in the slow cooling the condition of partial dissociation can be stereotyped by completing the cooling suddenly. This constitutes tempering.

Scheme of the Constants of the Rings.

J. Steel.—E, F, G, H, K. Soft Iron (Crown). I. Lowmoor.

Rowland's I., "Burden's Best."

Ring.	A.	$\log \frac{\kappa}{\rho}$.	$\log f$.	$\frac{A\rho}{\kappa} \times C$ $\log C = 6 \cdot 1000$.	Maximum observed value of μ .
J. First soft state...	11	4.78936	.17595	225	461
„ Second soft state	.129	2.82178	.16552	245	423
„ First hard state..	.01735	2.16327	.13925	150	157
„ Second hard state	.0236	2.46312	.15340	102	145
E3856	2.32208	.16153	2312	2234
F189	2.01524	.16151	2297	1895
G {	.35	2.23407	.15778	2570	2501
..... {	.31	2.11959	.15772	2963	
H25	2.24075	.15325	1808	1797
K39	2.30682	.15973	2422	2070
I217	2.06175	.16046	2370	2173
Rowland's Table I.	.343	2.14941	.16079	2666	2472

J. *First Hard State.*

$$A = \cdot 01735, \quad \log \frac{\kappa}{\rho} = 2 \cdot 16327, \quad \log f = \cdot 13925.$$

$$\mathfrak{B}_{\infty} = 20,000.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
13	$59^{\circ} 56'$	$82^{\circ} 35'$	45	49	- 5
59	$59 39$	$82 11$	47	47	0
121	$59 17$	$81 41$	50	49	+ 1
170	$59 0$	$81 18$	52	50	+ 2
268	$58 26$	$80 31$	56	57	- 1
405	$57 40$	$79 38$	62	62	0
551	$56 50$	$78 19$	68	64	+ 4
645	$56 20$	$77 38$	72	67	+ 5
2189	$48 40$	$67 4$	120	115	+ 5
4492	$40 8$	$55 18$	153	150	+ 3
6496	$34 39$	$47 45$	157	157	0
8959	$29 36$	$40 47$	145	134	+ 11
13870	$22 55$	$31 35$	91	70	+ 21
16292	$20 36$	$28 23$	57	56	+ 1
19533	$18 0$	$24 48$	7	53	- 45

J. *Second Soft State.*

$$A = \cdot 129, \quad \log \frac{\kappa}{\rho} = 2 \cdot 82178, \quad \log f = \cdot 16552.$$

$$\mathfrak{B}_{\infty} = 20,000.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
44	$59^{\circ} 56' 30''$	$87^{\circ} 45'$	101	92	+ 9
348	$59 32 52$	$87 10$	125	125	0
1110	$58 35$	$85 46$	180	188	- 8
1464	$58 7$	$85 5$	205	201	+ 4
1640	$57 55$	$84 47$	215	211	+ 4
2377	$57 0$	$83 27$	259	261	- 2
3701	$55 24$	$81 6$	325	323	+ 2
6734	$52 0$	$76 7$	411	422	- 11
9957	$48 43$	$71 19$	415	423	- 8
10029	$48 40$	$71 15$	413	353	+ 60
13252	$45 42$	$66 54$	341	334	+ 7
14470	$44 40$	$65 23$	295	227	+ 68
16118	$43 20$	$63 26$	224	173	+ 51
19198	$41 0$	$60 1$	52	61	- 9

J. Second Hard State.

$$A = \cdot 0236. \quad \log \frac{\kappa}{\rho} = 2 \cdot 46312, \quad \log f = \cdot 15340.$$

$$\mathfrak{B}_{\infty} = 20,000.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
43	59° 52' 20"	85° 14'	39	39	0
204	59 33 40	84 33	44	39	+ 5
538	58 26	83 11	54	46	+ 8
821	57 36	82 0	63	58	+ 5
923	57 20	81 37	66	59	+ 7
1308	56 15	80 5	76	72	+ 4
1489	55 46	79 24	80	77	+ 3
1572	55 32	79 4	82	78	+ 4
1767	55 1	78 18	87	81	+ 6
2186	53 55	76 46	96	106	-10
2477	53 10	75 41	102	113	-11
3655	50 20	71 39	121	130	- 9
4305	48 50	69 31	130	133	- 3
4468	48 30	69 3	131	136	- 5
4822	47 43	67 56	134	142	- 8
5442	46 26	66 6	139	142	- 3
6255	44 50	63 50	143	143	0
7351	42 48	60 56	145	145	0
8793	40 24	57 31	142	142	0
11128	36 58	52 38	124	123	+ 1
12631	35 2	49 52	112	97	+15
14093	33 20	47 27	94	86	+ 8
15515	31 50	45 19	74	66	+ 8
18276	29 16	41 40	30	48	-18

F.

$$A = \cdot 189, \quad \log \frac{\kappa}{\rho} = 2 \cdot 01524, \quad \log f = \cdot 16151.$$

$$\mathfrak{B}_{\infty} = 20,000.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
15	59° 52' 30"	86° 51'	208	208	0
143	58 49	85 19	306	360	- 54
513	55 40	80 45	592	587	+ 5
2784	42 0	60 55	1582	1592	- 10
3616	38 22	55 39	1747	1784	- 37
4283	35 48	51 56	1832	1869	- 37
5747	31 15	45 20	1894	1895	- 1
6438	29 27	42 43	1883	1825	+ 58
7655	26 45	38 48	1818	1704	+104
10767	21 40	31 26	1489	1581	- 92
12537	19 34	28 23	1241	1252	- 11
13900	18 10	26 21	1033	1000	+ 33
15035	17 11	24 55	853	692	+161
18834	14 31	21 3	206	150	+ 56

G.

$$A = \cdot 35,$$

$$A = \cdot 31,$$

$$\log \frac{\kappa}{\rho} = 2.23407, \log f = \cdot 15778.$$

$$\log \frac{\kappa}{\rho} = 2.11959, \log f = \cdot 15572.$$

$$\mathfrak{B}_{\infty} = 18,000.$$

$$\mathfrak{B}_{\infty} = 18,000.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.	μ .		Diffs.
			Calc.	Obs.		Calc.	Obs.	
51	59° 44' 40"	85° 55'	447	461	- 14	446	461	- 15
448	57 48	83 7	736	765	- 29	778	765	+ 13
1972	51 3	73 25	1601	1615	- 14	1738	1615	+123
6364	37 26	53 50	2403	2501	- 98	2423	2501	- 78
9003	33 6	47 36	2123	2241	-118	2136	2241	-105
11090	28 50	41 28	1812	1763	+ 49	1745	1763	- 18
11970	27 38	39 44	1623	1555	+ 68	1553	1555	- 2
13710	25 30	36 40	1204	1192	+ 12	1140	1192	- 52
14426	24 46	35 37	1017	832	+185	974	832	+142
15505	23 41	34 3	724	664	+ 60	680	664	+ 16
16042	23 11	31 54	562	395	+167	537	395	+142
17536	21 53	31 28	138	145	- 7	129	145	+ 16

H.

$$A = \cdot 25, \log \frac{\kappa}{\rho} = 2.24075, \log f = \cdot 15325.$$

$$\mathfrak{B}_{\infty} = 18,000.$$

\mathfrak{B} .	θ .	δ .	μ .		Diffs.
			Calc.	Obs.	
34	59° 50'	85° 9'	380	395	- 15
287	58 36	83 24	509	418	+ 91
1079	54 56	78 11	866	817	+ 49
6025	38 29	54 56	1727	1797	- 70
7705	34 46	49 19	1678	1717	- 39
8436	33 22	47 29	1616	1710	- 94
9740	31 6	44 16	1479	1354	+125
11261	28 50	41 2	1271	1134	+137
13355	26 10	37 14	903	885	+ 18
14501	24 55	35 28	712	630	+ 82
14718	24 40	35 1	672	502	+170
15409	24 0	34 9	536	254	+282
17642	22 0	31 18	76	97	- 21

K.

$$A = \cdot 39, \quad \log \frac{\kappa}{\rho} = 2 \cdot 30682, \quad \log f = \cdot 15973.$$

$$\mathfrak{B}_{\infty} = 15,500.$$

$\mathfrak{B}.$	$\theta.$	$\delta.$	$\mu.$		Diffs.
			Calc.	Obs.	
67	59 42 45	86 16	391	422	- 31
293	58 46	84 53	529	428	+101
2175	51 36	74 32	1386	1288	+ 98
2337	51 2	73 43	1439	1176	+263
3949	46 0	66 27	1800	1766	+ 34
4710	43 53	63 23	1982	1885	+ 97
5720	41 21 30	59 45	1922	2070	-148
8677	35 17	50 58	1676	1914	-238
8890	34 54	50 25	1643	1775	-132
9598	33 43	48 42	1519	1557	- 38
10413	32 27	46 52	1356	1531	-175
11511	30 52 30	44 36	1107	1104	+ 3
12148	30 1	43 22	950	799	+151
13246	28 40	41 25	659	666	- 7
13104	28 50	41 39	698	505	+193
13671	23 10	40 41	541	358	+183
15053	26 40	38 31	136	135	+ 1

I.

$$A = \cdot 217, \quad \log \frac{\kappa}{\rho} = 2 \cdot 06175, \quad \log f = \cdot 16046.$$

$$\mathfrak{B}_{\infty} = 20,000.$$

$\mathfrak{B}.$	$\theta.$	$\delta.$	$\mu.$		Diffs.
			Calc.	Obs.	
33	59 45 "	86 27	268	271	- 3
429	56 52 30	82 18	569	617	- 48
1322	51 5	73 55	1123	1024	+ 99
1624	49 20	71 23	1273	1427	-154
2395	45 15	65 29	1585	1558	+ 27
4541	36 33	52 53	2024	2072	- 48
6324	31 25	45 27	2082	2080	+ 2
6712	30 29	44 6	2071	1993	+ 78
6700	30 30	44 8	2071	2033	+ 38
7091	29 37	42 51	2054	2173	-119
8194	27 21 30	39 35	1974	1935	+ 39
9691	24 47	35 52	1813	1842	- 29
10400	23 43 30	34 20	1720	1483	+237
13159	20 20	29 25	1293	1188	+105
15050	18 32	26 49	958	818	+140
16309	17 30	25 19	724	592	+132
16939	17 0	24 36	604	444	+160
19303	14 34	21 5	141	155	- 14

Rowland's Table I.

$A = \cdot 343, \log \frac{\kappa}{\rho} = 2 \cdot 14941, \log f = \cdot 16079. \mathfrak{B}_{\infty} = 17,500.$

$\mathfrak{B}.$	$\theta.$	$\delta.$	$\mu.$		Diffs.
			Calc.	Obs.	
71·5	59 34 ⁶	86 15 ⁰	391	391	0
600·5	56 27	81 44	833	869	— 36
966·7	54 27	78 51	1097	1129	— 32
2460	47 12	68 21	1903	1936	— 33
2923	45 17	65 34	2068	2078	— 10
3082	44 38	64 38	2119	2124	— 5
4959	38 15	55 23	2444	2433	+ 11
5482	36 45	53 13	2468	2470	— 2
5782	35 56	52 2	2473	2472	+ 1
6651	33 47	48 55	2445	2448	— 3
7473	31 58	46 17	2371	2367	+ 4
8943	29 8	42 11	2175	2208	— 33
10080	27 16	39 29	1964	1899	+ 65
12270	24 16	35 8	1467	1448	+ 19
12970	23 24	33 53	1290	1269	+ 21
13630	22 42	32 52	1115	1137	— 22
14540	21 45	31 30	866	824	+ 42
15770	20 37	29 51	515	462	+ 53
16270	20 12	29 15	368	354	+ 14
16600	19 49	28 42	271	258	+ 13

XXXVIII. On the Seat of the Electromotive Forces in the Voltaic Cell. By Professor OLIVER J. LODGE, D.Sc.

[Concluded from p. 280.]

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19. H AVING now explained why I believe the main part of the Volta-effect to take its rise at the surface of	

* Very little of this chapter, and none of Chapter V., has yet been read anywhere.

contact between metal and medium, rather than between metal and metal, it remains to consider whether this belief requires one to assert that there is no true contact-force at all at the junction of two metals. By no means : the existence of such a force is undoubted ; but for metals it is usually very small and may be neglected in comparison with the Volta-force, though, strictly speaking, what is observed electroscopically is a mixture of the two. It is the true contact-force which gives rise to the Peltier effect, and its variation with temperature (assisted by the Thomson effect) causes thermoelectric currents. A contact-force exists, as Thomson has shown, not only at the junction of two different metals, but also between parts of the same metal at different temperatures.

In another place* I have endeavoured to gain some insight into the nature of this true contact-force and to suggest its cause. This has been done by many others ; but I may be permitted to repeat my own notion, vague and incomplete though it avowedly is. Molecules of matter do not move in independence of electricity ; at any rate the converse is certainly true—electricity does not move independently of matter. Electricity, in flowing through a wire, meets with resistance ; there is something analogous to friction between the matter and the electricity, and the opposing force is precisely proportional to the strength of the current. This much is expressed by Ohm's law, $E=RC$, which is a carefully verified though empirical statement. But, analyzing R into specific resistance of material (ρ) and sectional area of conductor, and

permitting ourselves to regard $\frac{C}{\text{area}}$ as proportional to the velocity of electricity in a circuit of different thicknesses, we perceive that Ohm's law means that

$$\frac{dV}{dx} = \rho \times \text{velocity}.$$

Let us then postulate, between electricity and any given kind of conducting matter, a connection which shows itself as an E.M.F. proportional to the speed of their relative motion and to the specific resistance of the material. Molecules of matter are not at rest, but (say) vibrating at a rate depending on, or rather itself determining, the temperature. These motions, as we have seen, cannot be independent of electricity, but they result in no force urging it to flow ; because their motions are symmetrical. But place two metals in contact—one hot, the other cold ; or one copper, the other iron : at the junction

* Phil. Mag. December (Supplement) 1876, "On a Mechanical Illustration of Thermoelectric Phenomena."

symmetry disappears, there must be constraint and accommodation; and, in whatever precise way this acts, it seems probable that it can be conceived of as having the same effect as a layer of molecules moving faster on their outward journey than on their return. If any such dissymmetry of velocity were produced, it would exert a propelling force on electricity* in the direction of the greatest velocity, because the force is proportional to the velocity. This is the crude and tentative way in which I picture to myself the Seebeck or true contact-force—the cause of thermoelectricity and of the Peltier phenomenon.

But now, why is this force so small in ordinary metals? Because it depends on ρ , the specific resistance, and this is small. Choose badly conducting metals like bismuth and antimony, or still better selenium and tellurium, and the force will be greatly increased. Choose so-called nonconductors, like glass and silk and ebonite, and it becomes enormous. But when one uses nonconductors we cannot expect to excite currents flowing in closed circuits; we can only expect electrical displacement and electrostatic phenomena; and indeed it is no such easy matter for electricity to move in such substances, even though the force urging it be excessive; and a little mechanical violence (friction) may be necessary to help it to move. But remember that no amount of friction can determine the motion in one direction rather than another: working a pump-piston exhausts no air unless there are valves. Friction may supply some of the energy, but the directing force must be in the substances in contact†. To assist the passage it is customary in electrical machines to touch together a conductor and insulator rather than two insulators. I doubt not that when metal touches glass the surface of contact would become chilled as soon as any transfer of electricity were really produced by the force; but the heat developed, by the friction apparently necessary to aid the transfer, effectually masks any chilling.

Measurement of contact-force between insulators is beset

* I do not say necessarily on *positive* electricity. It seems a complication; but Sir William's researches show that it is positive in some metals and negative in others. In the case of lead only does the grip on both electricities seem the same.

† Mr. Joseph Thomson (Proc. Roy. Soc. 1876) endeavoured to extend ordinary contact methods to nonconductors. He was hardly likely to get very clear results; but he was able to find some electrical transfer as the result of mere contact, if it be admitted that it is possible to apply mere contact and no sort or kind of violence—a supposition which is probably inadmissible. Yet the least violence destroys all novelty, and sends us back to Thales.

with difficulties, because it is so difficult to make electricity pass across the junction. No limit to the force has at present been observed: whenever an electrical machine reaches its limit, and refuses to charge its prime conductor or a Leyden jar to a higher potential, it is accounted for by saying that the rate of leakage is now equal to the rate of production (which is undeniably true), but nothing is said about whether the rate of production is the same as it was when the jar was uncharged. It is a difficult matter to settle, because most of the leakage takes place close to the rubber; and, though it is quite possible, it is unlikely that a limit to the force will be discovered, by finding the activity of a frictional machine less at high potentials than at low. When the substances in contact are two metals, it is impossible for them to drive electricity very hard, for it would, so to speak, slip through their fingers; but when an insulator is concerned, its grip is so great that probably there is no limit to the force until its insulating power is overcome, and through it also electricity begins to slip. Certainly any upper limit must be a very high one, for the force can readily pile up a charge till it produces sparks a foot or more long.

Whether *Volta* forces, or contact-forces between substances and the medium surrounding them, exist for insulators also we do not know; we have no reason whatever to deny their existence; but whereas in the case of metals these exceeded the forces acting between the substances themselves, here in the case of insulators they are absolutely negligible by comparison. For intermediate substances they may have correspondingly important values; and it seems not unlikely that at the junction of metals with electrolytes, and of electrolytes with one another, the total contact-force may be a complex one—partly chemical, and due to the possibilities of chemical action straining across the junction, and partly physical, due to different velocity of the molecules.

20. The preliminary experiments of Bouty have caused him to believe in the existence of physical contact-forces, at the junction of metals with electrolytes, which cannot be brought into harmony with energies of chemical action. And though the subject is too unexplored in this direction to be ripe for discussion, it may be well to point out that these contact-forces are important in the theory of the voltaic cell even in its simplest form.

Why is the E.M.F. of a zinc-copper battery less than that of a zinc-platinum?

Why is the E.M.F. of a zinc-lead or iron battery smaller than either?

The same chemical action goes on in each, zinc is dissolved at one end and hydrogen liberated at the other: how, then, can the E.M.F. be different if it is calculable from the chemical reactions?*

If we picture to ourselves the actual forces in action we shall get a kind of answer indicated to us. In a zinc-iron cell the E.M.F. is due to the zinc pulling at oxygen harder than the iron does; but, since the iron does pull too, with no inconsiderable strength, the balance of force is not so great as if the iron were replaced by copper, which pulls less, or by platinum, which barely pulls at all until it is coated and alloyed with hydrogen.

This answer cannot be considered as complete, and in order to complete it consider a more precise experiment.

Arrange a series of common dilute acid voltmeters, with their plates respectively, zinc zinc, zinc iron, zinc copper, and zinc platinum. Pass one current through the series, from zinc to the other metal, and measure the differences of potential between the plates in each cell. Now the same chemical action is going on in each. In each, zinc is dissolved at one side and hydrogen evolved at the other—the only difference being that it is liberated from surfaces of zinc, iron, copper, platinum, in the four cases. What is to prevent the E.M.F. between the terminals of each voltmeter from being the same? But it is not the same (*pace* Prof. Exner): the zinc-zinc cell shows the greatest difference of potential between its terminals, the zinc-iron less; and the zinc-platinum may easily show a reverse difference because it helps the current on instead of hindering it. It will be understood that the *precise* behaviour of the cells is determined by the intensity of the current (*i. e.* current per area)—if it is weak, even the zinc-iron cell may help it on, but the zinc-platinum will help it on most: if it is very strong, even the zinc-platinum will retard it, but the others will retard it more, and the zinc-zinc most.

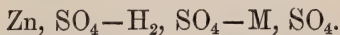
Now why is all this? Take the difference between the heats of formation of Zn , SO_4 and of H_2 , SO_4 , at the comma, and you will have the total energy assimilated by the current

* Professor Exner cuts this knot in characteristic fashion by asserting roundly that the E.M.F. of all such cells is the same, and that it matters nothing what metal is opposed to the zinc of a cell so long as it does not alter the chemical action going on. He further asserts that all batteries are non-polarizable and quite constant, as soon as they have got rid of dissolved air and before sulphate of zinc has accumulated. He verifies these extraordinary statements, to three significant figures, by straightforward experiment. See his paper "On Inconstant Voltaic Batteries," cited above.

in each cell. This energy is the same in all the cells, but not in all does it take the same form. In the zinc-platinum cell it mainly results in driving the current forward. In the zinc-zinc cell it wholly results in a Peltier (or Bouty) generation of heat. In going out of the cell by a cathode zinc plate, it has to move hydrogen towards it, and (*ipso facto*) oxygen away from it, in opposition to the strong chemical attraction; thus it will do work and liberate energy, which, since there is nothing better to do, must exhibit itself as heat. At an iron surface less heat is generated, and at a copper less still; but, at any cathode which attracts oxygen, some heat must be generated by a current made to do work in opposition to this attraction.

In the zinc-zinc cell there is no propulsion of electricity at all by the cell; on one side, where the current enters, zinc is dissolved and the current helped forward with the full energy (or nearly the full energy) of the combination, so that no (or nearly no) waste energy or heat is there produced; but on the other side, where the current leaves, the same combination is (not exactly undone but) opposed and the current hindered with (probably something less than) the full energy of the combination, and there the heat of combination is generated.

Thus, regarding the passage of hydrogen to the cathode as a virtual separation of O (or SO_4) from it, we may say in general that in any one of the above cells used as a voltmeter, the energy available for helping the current on is that represented by the difference between the combination-energies of the substances respectively attacked and liberated; *i. e.* $\text{Zn}, \text{SO}_4 - \text{H}_2, \text{SO}_4$; but that besides this, the combination M, SO_4 is virtually undone; and, since the energy of this combination appears as a generation of heat at the cathode, it is so much to be subtracted from the propelling force available for the current, only the balance being left for this purpose, *viz.*,



Whether one ought to write SO_4 or O in these expressions I am not sure, but it is not essential to decide this at present.

Another way of regarding the matter is to say that the force propelling the current is that due to the difference of energies $\text{Zn}, \text{O} - \text{M}, \text{O}$, but that as soon as a current actually passes and hydrogen is liberated it coats the cathode more or less thickly, and an extra term must be subtracted from the above to represent the opposition force of this hydrogen. The efficacy of this hydrogen as a current-opposer must

depend in some way on the intensity of the current itself, since with a feeble current it will be able to dissipate itself faster than it forms, and with a strong current it will thoroughly coat the plate and the balance will escape. Suppose, then, we represent the force exerted by the hydrogen as $H_2O f(C)$; where $f(0)=0$, and $f(\infty)=1$ or something like 1; then the force available for urging the current forward in any of the above cells is, in volts,

$$\frac{1}{46000} \{Zn, O - M, O - H_2O f(C)\}.$$

There is an obvious objection to be taken to this last hypothesis, viz. that it supposes the second metal M completely operative, even though it be thoroughly coated with hydrogen. This is hardly reasonable; and a compromise between the two preceding hypotheses is afforded by one of greater generality in which the available force is symbolically represented by

$$Zn - M \phi(C) - H_2 f(C),$$

where ϕ is a function such perhaps that $\phi(C) = 1 + n \cdot f(C)$.

On this hypothesis the propelling E.M.F. is

$$e = \frac{1}{46000} \{Zn, O - M, O - (H_2O + n \cdot M, O) f(C)\}.$$

This is too much like miscellaneous guessing, and we will make no more of such hypotheses; but if experiment could fix an empirical formula for this force in any case, we could apparently at once obtain the Joule or Bouty effect,* or rather the difference of two such effects, for that case; because we should have the E.M.F. experimentally observed on the one hand, and that calculated from pure energy-considerations on the other, as

$$\frac{Zn, O - H_2, O}{46000} + B_M - B_Z,$$

where the two B's stand for the Bouty coefficients at the zinc and the other metal respectively. The only objection is that in the cells now under discussion M is coated more or less with hydrogen, and hence the Bouty effect obtained is nothing very easily definable.

* That is the thermoelectric contact-force at a metal-liquid junction: see section 6.

21. To see if the actual behaviour of such cells at all bears out a hypothesis formed on the above plan, I have made some rough experiments on the lines suggested ; that is, I passed the same current through different simple cells. There are so many sources of uncertainty and of variation, that it would be very difficult to get really definite and reliable results. Thus, for instance, the back E.M.F. will depend considerably upon how long the current has been flowing, and so the readings will differ according to the time they are taken. The metals I used were zinc-zinc, zinc-copper, and zinc-platinum ; and it was found necessary to put the cathode-plate in a porous cell to avoid deposition of zinc on it. But it was now difficult to compare the cells easily when arranged in series, because different porous pots had different resistances. I therefore ultimately decided to use the same porous pot and the same anode zinc plate, and to substitute the other plates one after the other, making the current as nearly the same each time as convenient (by adjusting resistance) and allowing for outstanding discrepancies. An amperemeter placed in circuit measured the current, and the voltameter used was a reflecting galvanometer with some 30,000 ohms in its circuit. Its indications were interpreted absolutely by tapping off, at the same time as the cells, the difference of potential between the terminals of an ohm (or $\frac{1}{4}$ or $\frac{1}{8}$ ohm) coil placed in the circuit.

Any two values of the strength of current enabled the internal resistance of the cell to be calculated, provided its E.M.F. remained constant. With low currents it did seem to be fairly constant, and a mean value of the internal resistance r is reckoned from these as $\frac{1}{2}$ ohm.

The area of each plate under the liquid was exactly the same, and measured 3 inches by $2\frac{3}{4}$ inches. Both faces of each plate were exposed, though naturally one face was more active than the other.

The arithmetical reductions are rather long ; the results are all that I give. It will be perceived they are anomalous in places, a great deal of this being dependent on whether the reading of E.M.F. was taken soon after a current-change or not. As I said before, the plan of experiment is avowedly rough, though the actual readings were carefully taken ; but without understanding more about the circumstances of the case, and what possibilities of variation there are, I do not see how to plan a perfect system of experiment on the subject.

I will first give relative numbers, simply comparing the differences of potential between the terminals of the three

cells when the same current is going through each, the resistance of each being the same, viz. $\frac{1}{2}$ ohm; and then I will interpret the observations absolutely, calculating the E.M.F. of the cell under different currents, and seeing what empirical formula will best fit it.

Relative Differences of Potential between the Terminals of three Voltmeters of the same Resistance, through each of which the same current is driven by an auxiliary battery. Anode of each cell, zinc; cathode—zinc, copper, and platinum respectively.

Current flowing through each cell, in amperes.	Deflection of Electrometer attached to the terminals of		
	the Zinc-Zinc cell.	the Zinc-Copper cell.	the Zinc-Platinum cell.
.55	-125	+70 to +90	+ 87
.94	-148	+ 26	+ 40
1.88	-486	- 15	+ 50
1.5	-440	+ 15	+110
2.4	-535	- 65	+ 15
2.0	-486	- 8	+ 80
2.4	-626	-140	-100
1.96	-575	- 77	- 30
3.2	-700	-225	-201
2.73	-630	-250	-116
4.2	-800	-347	-335
3.4	-741	-250	-210

In the above Table the difference of potential between the terminals is written negative when it opposes the current, and positive when it helps it on.

We will now interpret similar measurements absolutely, reckoning the actual E.M.F. of each cell, and try to fit an empirical formula to it on the plan of those (in sect. 20) already guessed; assuming $f(C)$ a linear function, for simplicity, until forced to try something more complex. It is quite impossible that $f(C)$ can be a linear function really, but it very likely begins by being so, and only for big currents diverges notably. A hyperbolic tangent function, at a guess, would seem most likely to represent the case properly.

Electromotive Force of a *Zinc-Zinc*, Dilute Sulphuric-Acid, Cell, of Resistance $\frac{1}{5}$ ohm, through which the specified currents are driven by two or three Grove's; each plate exposing 53 square centimetres on each face.

Current, in amperes, C.	Difference of potential between terminals, in volts.	Observed E.M.F. of cell (obtained by adding rC to the preceding column).	E.M.F. calculated from the formula $e = -\frac{1}{2}C$.
·552	— ·37	— ·26	— ·276
·424	— ·34	— ·25	— ·212
·936	— ·44	— ·25	— ·468
·744	— ·40	— ·25	— ·372
1·88	—1·46	—1·08	— ·94
1·5	—1·32	—1·02	— ·75
2·4	—1·61	—1·13	—1·20
1·97	—1·46	—1·07	— ·99
2·4	—1·88	—1·40	—1·20
1·94	—1·80	—1·41	— ·97
3·22	—2·10	—1·46	—1·61
2·68	—1·88	—1·34	—1·34
3·36	—2·22	—1·55	—1·68
4·24	—2·40	—1·55	—2·12

The agreement between the observed and calculated columns is not very bad; and the polarization E.M.F. does not show decided signs of breaking away from the law of simple proportion until a current-strength of 4 amperes is reached—say an intensity of ·04 ampere per square centimetre of total surface.

Electromotive Force of a *Zinc-Copper*, Dilute Sulphuric-Acid, Cell, of Resistance $\frac{1}{5}$ ohm, through which the specified currents are driven by two or three Grove's; each plate exposing 53 square centimetres on each face. E.M.F. reckoned positive when it helps the current forward, negative when it opposes it.

Current, in amperes, C.	Difference of potential between terminals, in volts.	Observed E.M.F. of cell (obtained by adding rC to the preceding column).	E.M.F. calculated from the formula $e = \cdot 8 - \frac{1}{4}C$.	E.M.F. calculated from the empirical formula $e = \cdot 58 - \frac{1}{8}C$.
·55	+ ·24	+ ·35	+ ·66	+ ·47
·94	+ ·078	+ ·27	+ ·54	+ ·39
·76	+ ·108	+ ·26	+ ·61	+ ·43
1·88	— ·045	+ ·33	+ ·33	+ ·20
1·52	+ ·045	+ ·35	+ ·42	+ ·27
2·4	— ·195	+ ·28	+ ·20	+ ·10
2·0	— ·024	+ ·38	+ ·30	+ ·18
2·4	— ·42	+ ·06	+ ·20	+ ·10
1·98	— ·221	+ ·18	+ ·30	+ ·18
3·22	— ·675	— ·031	— ·05	— ·06
2·75	— ·75	— ·2	— ·11	+ ·03
4·16	—1·04	— ·21	— ·24	— ·25
3·44	— ·75	— ·06	— ·06	— ·11

Here two alternative formulæ are given ; it is a matter of opinion which shows the least divergencies from the column of observed values.

The first is the one most naturally suggested by the theoretical considerations of sect. 20, the $\cdot 8$ standing for $\text{Zn/O} - \text{Cu/O}$, or what is commonly called Zn/Cu .

Electromotive Force of a *Zinc-Platinum*, Dilute Sulphuric-Acid, Cell, of Resistance $\frac{1}{5}$ ohm, through which the specified currents are driven by two or three Grove's ; each plate exposing 53 square centimetres of surface on either side. E.M.F. reckoned positive when it helps the current on.

Current, in amperes, C.	Difference of potential between terminals, in volts.	Observed E.M.F. of cell (obtained by adding rC to the preceding column).	E.M.F. calculated from the empirical formula $e = \cdot 4 - \cdot 1 C$.	E.M.F. calculated from the formula $e = 1\cdot 2 - \frac{1}{5} C$.
$\cdot 552$	$+ \cdot 26$	$+ \cdot 37$	$+ \cdot 35$	$+ 1\cdot 02$
$\cdot 44$	$+ \cdot 30$	$+ \cdot 39$	$+ \cdot 36$	$+ 1\cdot 05$
$\cdot 936$	$+ \cdot 12$	$+ \cdot 31$	$+ \cdot 31$	$+ \cdot 89$
$\cdot 784$	$+ \cdot 17$	$+ \cdot 33$	$+ \cdot 32$	$+ \cdot 94$
$1\cdot 88$	$+ \cdot 15$	$+ \cdot 52$	$+ \cdot 21$	$+ \cdot 57$
$1\cdot 52$	$+ \cdot 33$	$+ \cdot 63$	$+ \cdot 25$	$+ \cdot 69$
$2\cdot 4$	$+ \cdot 45$	$+ \cdot 93$	$+ \cdot 16$	$+ \cdot 40$
$2\cdot 0$	$+ \cdot 24$	$+ \cdot 64$	$+ \cdot 20$	$+ \cdot 53$
$2\cdot 4$	$- \cdot 30$	$+ \cdot 18$	$+ \cdot 16$	$+ \cdot 40$
$1\cdot 96$	$- \cdot 09$	$+ \cdot 30$	$+ \cdot 20$	$+ \cdot 55$
$3\cdot 22$	$- \cdot 60$	$+ \cdot 04$	$+ \cdot 08$	$+ \cdot 13$
$2\cdot 72$	$- \cdot 35$	$+ \cdot 20$	$+ \cdot 13$	$+ \cdot 29$
$4\cdot 16$	$- 1\cdot 0$	$- \cdot 17$	$- \cdot 16$	$- \cdot 19$
$3\cdot 36$	$- \cdot 63$	$+ \cdot 04$	$+ \cdot 06$	$+ \cdot 08$

Here also are two alternative formulæ given, of which the first agrees best with the experimental results. But it is very strange that the E.M.F. of this cell should be so low when the current is feeble : it is scarcely more than that of the copper cell. The only way I see of accounting for the error (if error it be) is that the platinum was put into the liquid after the copper plate, and it was sometimes found coated with a very thin evanescent film of copper when taken out. Theoretical considerations would suggest something more like the second formula as the probable E.M.F., the $1\cdot 2$ being what is ordinarily called Zn/Pt .

22. I can now continue the quotation of the remainder of the preliminary notes with the certainty that they will be at any rate intelligible. I begin with statements intended to be true for substances of every kind, and then specialize them for the case of metals.

III. *Statements believed by the writer to be true, though not entirely orthodox.*

xii. A substance immersed in any medium tending to act upon it chemically is (unless it is actually attacked) at a different potential to the medium in contact with it; positive if the active element in the medium is electro-positive, negative if the active element is electro-negative.

xiii. The above difference of potential can be calculated approximately from the potential energy of combination between the substance and the medium; the energy being measured by compelling the combination to occur, and observing the heat produced per amount of substance corresponding to one unit of electricity.

xiv. In addition to this contact-force, due to potential chemical action or chemical strain, there is another which is independent of chemical properties but which seems to be greatest for badly-conducting solids, and which is in every case superposed upon the former contact-force, the two being observed together and called the Volta effect. Very little is known about this latter force except in the case of metals; and in these it varies with temperature, and is small. In the case of non-metals it is often much larger than the chemical contact-force*.

xv. The total contact-force at any junction can be experimentally determined by measuring the reversible energy developed or absorbed there per unit quantity of electricity conveyed across the junction (practical difficulties, caused by irreversible disturbances, being supposed overcome†).

xvi. In a chain of any substances whatever, the resultant E.M.F. between any two points is equal to the sum of the true contact-forces acting across every section of the chain between the given points (neglecting magnetic or impressed forces).

xvii. In a closed chain the sum of the "Volta forces," measured electrostatically in any (the same) medium, is equal to the sum of the true contact-forces; whether each individual

* I here assume, what I suppose is recognized as true, that what is known as frictional generation of electricity is really due to a contact-force between the substances rubbed—a force which is exceedingly great for insulators (see § 19). Davy seems to have held this view, from a note on p. 50 of his Bakerian lecture in 1806, cited before.

† These difficulties are, however, tremendous for most substances except metals. M. Bouty's is the only attempt I know of to examine junction-energy between metals and solutions of their salts, which is the case next in simplicity to metals. Observe that the statement says *energy*, not heat only.

Volta force be equal to each individual true force or not. See section 7.

xviii. Wherever a current flows across a seat of E.M.F., there it must gain or lose energy at a rate numerically equal to the E.M.F. multiplied by the strength of the current*.

Development of the above and special application to Metals.

xix. A metal is not at the potential of the air touching it, but is always slightly below that potential by an amount roughly proportional to its heat of combustion, and calculable, at any rate approximately, from it. For instance, clean zinc is probably about 1·8 volt below the air, copper about ·8 volt below, and so on. If an ordinary oxidizing medium be substituted for "air" in the above statement it makes but little difference.

xx. Two metals put into contact reduce each other instantly to practically the same potential; and consequently the most oxidizable one receives from the other a positive charge, the effect of which can be observed electrostatically.

xxi. There is a slight true contact-force at the junction of two metals which prevents their reduction to *exactly* the same potential; but the outstanding difference is small, and varies with temperature. It can be measured thermoelectrically by the Peltier effect, but in no other known way. It is probably entirely independent of surrounding media, metallic or otherwise†.

xxii. If two metals are in contact, the potential of the medium surrounding them is no longer uniform: if a dielectric it is in a state of strain, if an electrolyte it conveys a current.

xxiii. In the former case the major part of the total difference of potential is related closely to the difference of the potential energies of combination, and is approximately calculable therefrom. In the latter case the total E.M.F. is calculable accurately from the energy of the chemical

* A current gains energy at any junction at which heat is absorbed, or chemical combination permitted, or any other form of energy destroyed, by the passage of the current. The current gains the energy which has in the other form disappeared.

A current loses energy at a point where it causes other forms of energy to make their appearance; *e. g.* generation of heat, decomposition of chemical compounds, &c.

† To distinguish between Peltier-force and Volta-force henceforward it will be best to write Bi/Sb or Zn/Cu for the former, and Zn/Air/Cu or Fe/Water/Pt for the latter. The force electroscopically observed is Air/Zn/Cu/Air, but this involves both; the right way of denoting the Volta effect pure and simple is Zn/Air/Cu.

processes going on, minus or plus the energies concerned in reversible heat-effects*.

xxiii a. "The E.M.F. of an electro-chemical apparatus" whose energy is entirely expended in maintaining a current "is equal to the mechanical equivalent of the chemical action on one electro-chemical equivalent of the substance." (Thomson.)

xxiii b. "If the action in a cell consists in part of irreversible processes, such as (1) frictional generation of heat, (2) diffusion of primary or secondary products, (3) any other action which is not reversed with the current, there will be a certain dissipation of energy, and the E.M.F. of the circuit will be less than the loss of intrinsic energy corresponding to the electrolysis of one electro-chemical equivalent. It is only the strictly reversible processes that must be taken into account in calculating the E.M.F. of a circuit." (Maxwell: 'Elementary Electricity,' p. 148.)

xxiv. There are two distinct and independent kinds of series in which metals (and possibly all solids) can be placed: one kind depends on the dielectric or electrolytic medium in which the bodies are immersed, the other kind depends on temperature. The one is the real Volta series, but it is the commonly observed Volta series minus the Peltier; the other is the Peltier or thermoelectric series. To reckon up the total E.M.F. of a circuit, we may take differences of numbers from each series and add them together.

23. It is necessary to illustrate the meaning of this last statement, No. xxiv. By "real Volta series" I mean such series as we have attempted to calculate from purely chemical data, because they depend on chemical tendencies. By "Peltier or thermoelectric series" I mean those giving a purely physical E.M.F., produced we know not quite how, whose energy-source is not chemical but thermal. We have on the one hand a number of Volta series, each for a special medium, and on the other a table of thermoelectric powers at different temperatures. The latter can be conveniently represented by a number of curves, because temperature varies continuously; Volta series, on the other hand, can hardly be represented geometrically, because the transition from one medium to another is probably *per saltum*; at least, it is not

* Such, for instance, as we have been discussing under the head of inconstant or simple voltaic batteries (sects. 19-21). These reversible heat-effects indicate the presence of thermal contact-forces which, wherever they exist, prevent chemical data from giving E.M.F. accurately: they also must be taken into account. We have called them Joule or Bouty effects.

known what is the effect of mixing media, and so passing gradually from one to the next.

We have given several Volta series; and, for the sake of completeness, I will now give some Peltier series for a few substances according to the experiments of Professor Tait at different Centigrade temperatures. Expressing each number as a function of the temperature, we are able to give an infinite number of Peltier series in one table. The range of temperature over which this table may be interpreted is from -18° to 400° or so, provided the metals do not begin to melt. Non-metallic substances have not yet been introduced into such series: much experimental work remains to be done before they can be. The metals used by Tait were not chemically pure.

True Contact E.M.F. or Peltier Series. (Microvolts.)

Metals.	At any Temperature t° C.	At 10° C.	At 100° C.
Iron	$-4760 - 3.94t + .0487t^2$	-4795	-4667
Hard Platinum.....	$-718 - .54t + .0075t^2$	-722	-697
Soft Platinum	$+168 + 3.63t + .011t^2$	+205	+641
Magnesium	$-618 + .36t + .0095t^2$	-613	-487
German Silver	$+3310 + 26.17t + .0512t^2$	+3577	+6439
Cadmium	$-731 - 14.46t - .0429t^2$	-880	-2606
Zinc	$-643 - 8.95t - .024t^2$	-735	-1778
Silver.....	$-590 - 6.26t - .015t^2$	-654	-1366
Lead	0	0	0
Copper	$-374 - 3.96t - .0095t^2$	-415	-865
Tin.....	$+118 - 1.08t - .0055t^2$	+107	-45
Aluminium	$+211 - .31t - .0039t^2$	+207	+141
Palladium	$+1718 + 16.15t + .036t^2$	+1883	+3693
Hypothetical* Mercury } (Gauguin)	$+1800 + 4.6t - .007t^2$	+1845	+2190

To find the E.M.F. of a junction at a specified temperature we have only to subtract the numbers in the above table, inserting the value of the temperature. Thus a junction of zinc and copper at 10° has an E.M.F. of 320 microvolts, acting from copper to zinc; and a unit current sent across such a junction from copper to zinc, or from zinc to copper, absorbs or generates heat at the rate of 320 microwatts, and the current gains or loses energy at the same rate. Clerk-Maxwell says that the force is one microvolt, and that it acts from zinc to copper ('Elementary Electricity,' p. 149, note); but he only means, I suppose, that the E.M.F. of a zinc-copper circuit with one junction a degree hotter than the other is a micro-

* This row of numbers is little better than a guess from some curves given in Wiedemann's *Elektricität*. A more probable deduction from some quite new experiments of C. L. Weber (Wied. Ann. November 1884), gives, for mercury, $1181 + 5.68t + .005t^2$. (Cf. note to sect. 27.)

volt, and is such as to drive the current from zinc to copper across the cooler junction; at least this is true above -60° or -80° .*

Hitherto we have supposed the circuit to be all at one temperature; but if different parts are at different temperatures, we shall have to use a yet further series, viz. a Thomson series, for the E.M.F. acting in any one substance with a difference of temperature between its ends, or the force acting at a junction of two pieces of the same metal at different temperatures. This series can be deduced from the preceding, using only the coefficient of t^2 , and multiplying it by the difference of the squares of the *absolute* temperatures of the two ends of the piece of metal. Such a series then stands thus:—

Thomson Series, or E.M.F. in a metal whose ends differ in temperature. (Microvolts.)

Iron	$\cdot 0487 (t_1 - t_2) \left\{ 274 + \frac{1}{2}(t_1 + t_2) \right\}$
German silver . .	$\cdot 0512 (t_1 - t_2) \left\{ 274 + \frac{1}{2}(t_1 + t_2) \right\}$
Zinc	$-\cdot 024 (t_1 - t_2) \left\{ 274 + \frac{1}{2}(t_1 + t_2) \right\}$

and so on.

Whether a series of this sort can be made to include any non-metallic conductors also, has not yet been discovered. M. Bouty's experiments provisionally indicate the very interesting fact that Sir W. Thomson's general thermodynamic laws of the thermoelectric circuit apply perfectly to circuits which include some electrolytes as well as metals.

Now the meaning of statement No. xxiv. is as follows: Regard zinc and copper in contact as a circuit completed by air or by water, as the case may be, and let the temperature be uniform, and say 10° ; to reckon up the total E.M.F. we must look in the proper Volta series for Zn/air (or Zn/water), which we find $1\cdot 8$ say; for Zn/Cu, which we don't find, or find zero; for Cu/air, which we find $\cdot 8$. Then we must look in the 10° Peltier series for Zn/air or Zn/water, which at present we shall not find there for want of data (possibly we have no right to put them there if we had data); for Zn/Cu,

* It is always easy to tell from thermoelectric data which way the force acts at a junction; but it is not always the same way as the current flows, by any means. A current, excited by differences of temperature in a simple metallic circuit, may be urged against the force at *both* junctions. This is the case, for instance, in a copper-iron circuit with one junction above 275° and the other below it by a greater amount. It is customary to say that the current flows across a hot junction from the metal of higher to the metal of lower thermoelectric value: this is not necessarily true. The safe statement is to say that the electromotive force acts from high to low thermoelectric value, at either junction.

which we find about 320 microvolts ; and for Cu/air, which again we don't find. Add them all up with their proper signs, and we have the total E.M.F. of the circuit.

Again, consider the case of a Daniell cell at a given temperature producing a current ; we shall have to look in each series for Zn/ZnSO₄, for ZnSO₄/CuSO₄, for CuSO₄/Cu, and for Cu/Zn, and add them all up. It is true that these tables of numbers have practically yet to be made, for at present they include so few substances ; but that does not affect the question of the existence and independence of these two kinds of series.

It is, of course, a question how far *all* E.M.F. of contact may be found to depend on chemical tendency. For instance, when bismuth and antimony are put into contact, does the E.M.F. developed measure the alloying affinity of these two metals ? When sodium is dropped into mercury, does the heat produced represent the thermoelectric power of a sodium-mercury junction ? When metal touches glass, does the tremendous E.M.F. developed represent a tendency of the metal to combine with the glass ? These are questions for experiment to decide ; but to me it does not seem probable that it will reply in the affirmative.

We know that Sir W. Thomson, and Davy before him, considered the apparent contact-force at the junction of zinc and copper to be due to the chemical affinity between these two metals, and to be measured by the heat of formation of brass ; but this we have seen strong reasons for disbelieving. It sounds more probable that the real contact-force at a junction of bismuth and antimony should be due to the chemical affinity between these metals ; but perhaps it is no more true. The greater part of a contact-force of this kind is probably due to a physical difference between the metals, such as difference in atomic velocity, and has no close relation to their chemical affinities for each other. It is, however, just possible that *part* of a metallic junction-force is due to chemical tendency between the two metals in contact. For instance, take the case of zinc and copper. There is, I suppose, an undoubted affinity between them, as shown by the formation of brass under proper conditions. [If chemists assume the right to demur to this, on the ground that the two metals mix equally well in any proportions, one can choose any other pair of metals—say, perhaps, copper and tin—for which the statement does not hold.] Now does this affinity result in any E.M.F. between them on making contact ? This question, I apprehend, is to be answered by passing a current for a long time across a copper-zinc junction and seeing if any brass does,

after a long time, result. Thermopiles show a curious secular deterioration with use, and it may be that some alloying action goes on, though I have never heard of its being noticed. But if no such alloying goes on during the passage of a current, then I should say that, in whatever ways chemical affinity between two metals is able to show itself, it does not show itself as an E.M.F.

Observe, I do not for a moment question the existence of a few hundred microvolts of E.M.F. at a zinc-copper junction. I only ask, is this chemical, or is it physical, or is it a mixture of the two? Statement No. xxiv. is general enough to take into account the possibility of its being a mixture of the two at every kind of junction. It is easy to write one of them zero, if so it turns out.

24. We have been led into a pretty wide discussion of contact-force in general; and, before digressing again on the question of a contact-force-determination of the size of atoms, it may be convenient here to quote the remainder of my preliminary notes, which aim at summarizing, in a compact form, the main argument with respect to the immediate subject of discussion, viz. the seat of electromotive force in a voltaic cell, and in ordinary Volta-condenser experiments.

IV. *Brief Summary of the Argument.*

xxv. Wherever a current gains or loses energy, *there* must be a seat of E.M.F.; and conversely, wherever there is a seat of E.M.F. a current must lose or gain energy in passing it*.

xxvi. A current gains no appreciable energy in crossing from copper to zinc, hence there is no appreciable E.M.F. there.

* *Note added January 1885.*—My attention has just been called to an article by Mr. Oliver Heaviside in the 'Electrician' of 2nd February, 1884, in which he states views very like those contained in these statements. Had I known of this paper earlier I should of course have mentioned it, but I did not know of it.

Mr. Sprague also, in his book on 'Electricity, its Theory, Sources, and Applications,' on page 331 expresses his belief in much the same sort of way. Although Mr. Sprague is rather too much occupied in tilting against what he considers the absurdities of orthodox and "mathematical" views to work out his own ideas in a very thorough and clear form, there can be no doubt that he has in his own way arrived at very many of the same conclusions as Clerk-Maxwell, though of course in a far less profound and satisfactory manner. He has scarcely received the credit due to him for this, and a cutting review of his book in 'Nature' some years back, though justified in parts by Mr. Sprague's supercilious tone, yet falls into more important errors regarding electrical *facts* than Mr. Sprague himself falls into. See 'Nature,' June 24, 1875, vol. xii. p 144.

xxvii. When a current flows from zinc to acid, the energy of the combination which occurs is by no means accounted for by the heat there generated, and the balance is gained by the current; hence at a zinc-acid junction there must be a considerable E.M.F. (say at a maximum 2·3 volts).

xxviii. A piece of zinc immersed in acid is therefore at a lower potential than the acid, though how much lower it is impossible precisely to say, because no actual chemical action occurs. [If chemical action does occur, it is due to impurities, or at any rate to local currents, and is of the nature of a disturbance.]

xxix. A piece of zinc, half in air and half in water, causes no great difference of potential between the air and the water (Thomson, Clifton, Ayrton and Perry, &c.); consequently air must behave much like water.

xxx. If it makes the air slightly positive to the water, as it does (Hänel), it may mean that the potential-energy of combination of air with zinc is slightly greater than that of water, or it may represent a difference in the thermoelectric contact-forces between zinc and air and zinc and water, or it may depend on a contact-force between air and water. [If such a contact-force between air and water exists, it is obviously of great importance in the theory of atmospheric electricity, for the slow sinking of mist-globules through the air would render them electrical*.]

xxxi. Condenser methods of investigating contact-force no more avoid the necessity for unknown contacts than do straightforward electrometer or galvanometer methods; the circuit is completed by air in the one case and by metal in the other, and the E.M.F. of an air-contact is more hopelessly unknown than that of a metal-contact.

xxxii. All electrostatic determinations of contact-force are really determinations of the sum of at least three such forces, none of which are knowable separately by this means.

xxxiii. The only direct way of investigating contact-force is by the Peltier effect or its analogues. [Maxwell.]

xxxiv. Zinc and copper in contact are oppositely charged, but are not at very different potentials; they were at different potentials before contact, but the contact has nearly equalized them.

xxxv. The potential of the medium surrounding them is, however, not uniform. If a dielectric, it is in a state of strain; if an electrolyte, it is conveying a current.

* Cf. lecture on "Dust," Nature, 22nd January, 1885.

Chapter V. *Suggestive.*

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Size of Atoms.

25. I may now claim to have accomplished my task, and terminate this long paper; but there are several interesting points which arise in connection with Sir W. Thomson's deduction of a limit to the smallness of atoms from contact data, and these I may be permitted to indicate. Indeed it evidently becomes a question whether or not his argument remains quite valid if the chemical-strain view be taken of Volta's force.

Let us then inquire whether any modification has to be made in Sir Wm. Thomson's argument, if the hypothesis set forth in this paper be adopted. He says (virtually), Take a number of plates of zinc and copper of specified thickness, arrange them alternately like the leaves of a book with the covers doubled right back, and then shut the book. Directly they touched at one edge they became oppositely electrified and attracted each other, and therefore did work as they approached. By making the leaves numerous and thin enough, and shutting them up close enough, any required amount of work can be thus done with given quantities of metal, provided the thin plates retain the same properties as masses of metal possess; *i. e.* provided they are not only a few atoms thick. So far there is no possible objection; but Sir William proceeds to consider the attraction as depending on the affinity of zinc for copper, and the work he requires of his plates is that evolved in the formation of brass. But if we regard the attraction as depending on the difference of combustion energies, $\text{Zn/O} - \text{Cu/O}$, we must, to keep the charge constant, not only take the plates several atoms thick, but we must suppose films of air of sufficient thickness to preserve their normal activities in the way of chemical strain to be shut up with the plates. Given these, the amount of work he has calculated would certainly be done in shutting the book, and a corresponding amount of heat generated. But would this heat have anything to do with the making of brass? So far as I can see, nothing whatever.

If we intend to make brass, must we not regard the air surrounding the plates as a simple accident, and imagine all air-films removed before beginning the operation? Work with the zinc and copper plates in absolute vacuum, where (on my hypothesis at any rate) the only difference of potential between them is a minute thermoelectric one; there will be an attraction caused by this difference of potential, and work will be done in shutting the book, but to get any appreciable amount of heat the plates must be terribly thin. How much heat really is produced in the formation of brass I don't believe any one knows; but if it be enough to warm the metals sixty degrees, the lower limit to the size of atoms becomes greatly depressed.

In a note near the end of this paper I show that a rise of from $\frac{1}{4}$ to 2 degrees is all that is probable, on the usual estimate of atomic dimensions; the smaller evolution of heat being caused by alloying the metals at 10°C ., the larger being produced by alloying them at 400°C .

26. *Is there much heat produced in the formation of brass? Is there any way of attacking the question simply? The only way which has occurred* to me is to dissolve brass in acid, and to see whether one gets appreciably less heat than by dissolving its constituent copper and zinc separately. When an alloy is dissolved, I suppose the affinities of its constituents are unloosed, or the combination undone; hence the heat developed during the solution of an alloy, subtracted from that produced during the solution of its constituent metals and mixing of those solutions, ought to measure the heat of formation of the alloy. Dr. Forster Morley, of University College, London (also on the boat), said he might be willing to undertake this observation, which is doubtless a delicate one, for he was engaged in some thermo-chemical researches. It may not be practicable for the actual case of brass, because of the complication and uncertainty introduced by secondary products, but a better pair of metals may no doubt be readily found.*

Adhering to zinc and copper as convenient for explanation, the argument, though obviously not the order of experiment, will stand as follows:—Take definite weights of zinc and copper, dissolve them separately, getting heats H_1 and H_2 respectively; then mix the solutions, getting a possible further heat-production h . This is one plan of passing from separate zinc and copper to a solution of a salt of brass.

* It occurred in conversation with Professor S. P. Thompson and Dr. J. A. Fleming on board the Quebec excursion steamer 'Canada', and I am unable to say who suggested it.

Next take the same weights of zinc and copper as before and alloy them, getting heat H ; then dissolve the brass in the same acid as before, getting heat H_3 . This is another plan of passing from separate zinc and copper to a solution of a salt of brass.

Now, unless external work and secondary products are different in the two cases, we are justified in writing the heats evolved in the two cases equal:—

$$H_1 + H_2 + h = H + H_3.$$

H is the unknown quantity to be determined, and its determination involves four separate measurements, H_1 , H_2 , H_3 , h .

The only one of these at all easy to observe is h , and this my assistant, Mr. Butler, has done. Proportions of zinc and copper sulphate, containing equal weights of zinc and copper, are dissolved in as little water as will keep in solution any double salt that may be formed on mixing. The zinc sulphate is enclosed in a thin bulb or tube inside the other solution, and left, screened from stray heat, for some hours. The bulb is then broken, or the liquid otherwise blown out of it, and the liquids mixed. No certain change of temperature so great as a hundredth of a degree has been observed.

27. In thinking over what metals were more suitable, it struck me that the heat of formation of amalgams was a subject easy of direct attack. I therefore, as a preliminary, have dissolved a little granulated tin in mercury. Of course the latent heat of liquefaction of tin has to be allowed for, and the actually observed result is a cooling; but I hoped that the cooling observed would be less than what the latent heat would account for, and that I might then calculate the real evolution of heat due to combination. Unfortunately the only data I know of with reference to the latent heat of tin relate to its ordinary melting-point, at which point it is given by Rudberg as 13.3, and by Person as 14.25. We have no ground whatever for believing latent heat to be constant, and I am therefore utterly in the dark as to what the latent heat of tin at ordinary temperatures may be. That liquid tin could be supercooled to ordinary temperatures without solidification is unlikely. I give, however, the data of my experiment (which was carefully performed) in case better latent-heat data are known to some one else:—2.10 grammes of thin granulated tin at 12°4 were dropped into 502.00 grammes of mercury at a steady temperature of 10°85, contained in a large thin protected test-tube, of which the part sharing the temperature of the mercury weighed 8 grammes. After solution, which took ten minutes, the resulting temperature was found to be 8°82.

Three minutes later it had risen to 8.99 from surrounding influences. The thermal capacity of the immersed part of the thermometer was equivalent to .48 gramme of water.

Working on these data, and taking the specific heat of tin as .056, latent heat 14.25, specific heat of glass .19, and of mercury .033, we find:—

Heat disposed of in cooling and liquefying tin . 30.45 units.
Disappearance of heat actually observed . . . 43.57 „

More than can be accounted for without any combination-heat at all! This is rather depressing; but it only shows how wrong is the estimate of 14.25 for the latent heat of liquid tin at 10° C.

Ignorance of the true latent heat thus effectually prevents our obtaining any information whatever, about the heat of combination of tin and mercury, from the experiment. It seems indeed easier to observe the combination-heat by a process of dissolving the amalgam and the metals separately in acid, as already explained for brass, and then to use the above experiment to calculate latent heat from. One might perhaps thus get the latent heats of fusion at various temperatures for metals soluble in mercury.

Another alternative, however, presents itself. Instead of trying to reduce the latent heat to ordinary temperatures, one might form the amalgam at a temperature just below the melting-point of tin, and obtain, if possible, the net evolution of heat then.

Suppose the heat of combination of the 2.1 grammes of tin with mercury to be somehow or other determined, we have next to suppose the amalgam made otherwise, bringing the molecules together in a reasoned way. Let the same quantity of tin be brought to within molecular distance of the mercury in successive pieces of very thin foil, first made to touch at one corner and then laid down.

It is quite true that each flake would be charged with a Volta E.M.F. of, say, .6 volt, and so would attract the mercury and do a certain amount of work in laying itself down. But it is not fair to compare an operation thus conducted in air with the dropping of a solid mass of tin into mercury: to be able to compare the two operations, one must perform the foil experiment in absolute vacuum. This being done, the contact E.M.F. is no longer .6 volt, but only about .00015 volt according to the experiments of Matthiessen. Good data for this quantity are, however, wanting. Mercury is not one of the metals included in Professor Tait's series. It was observed by Gaugain; and by rather hypothetical deduction

from his numbers, as given pictorially in Wiedemann's *Elektricität*, I make the tin-mercury Peltier force 1.75 millivolt at 10° .*

Taking one of these numbers (15,000 or 175,000 in C.G.S. units), or a better one when determined, we can calculate how near the given mass of tin must be brought to the mercury in order to generate the actual heat of combination, provided one knows the specific inductive capacity of absolute vacuum†. But I do not know it. Thus the supply of data for this case is distinctly unsatisfactory.

28. Let us try whether we cannot do better with a single metal exposed to air, not troubling about the contact of two metals, which is unnecessary, but simply considering one metal in contact with air.

* Since this was in type, a paper by C. L. Weber has appeared in Wiedemann's *Annalen* for November 1884, on the thermoelectric properties of amalgams, in which mercury itself was examined; and from the data there recorded, together with Tait's value for copper, I reckon the thermoelectric value of mercury at t° C. as

$$431 + 5t \text{ absolute electromagnetic units.}$$

Whence the Peltier force at the same temperature is

$$1181 + 5.68t + .005t^2 \text{ microvolts.}$$

The Peltier force between tin and mercury at 10° is therefore 123,800 absolute units, or 1.24 millivolt, which agrees well enough with the rough estimate above.

† Taking this as 1, and assuming the estimate of molecular dimensions hereafter established, and working backwards, one can show that the Peltier force of tin and mercury at 10° is connected with the heat of combination of our 2.1 grammes of tin with the 502 grammes of mercury by the relation

$$J\Pi = 3.6 \times 10^6 \sqrt{H}.$$

The two rough estimates of $J\Pi$ deduced from Matthiessen and Gauguin respectively (15,000 and 175,000) thus give H as about $\frac{1}{4}$ and $\frac{1}{16}$ of a unit respectively. Either of these is too small a quantity to be observed in the process of dissolving tin in mercury; so neglecting it we get, from that experiment, the latent heat of molten tin at 10° C. as 20.4. Another experiment made in a similar way gave 19.6.

If the above reasoning be regarded as legitimate, a combination of thermoelectric measurements with observed heats of solution in mercury may furnish a means of estimating latent heats of fusion at various low temperatures in general.

Working back similarly to the heat of combination of 1 gramme of copper with 1 gramme of zinc, we calculate .077 unit as the heat developed at ordinary temperatures; only enough to raise the mass of brass formed through three eighths of a degree Centigrade. At a higher temperature, such as 400° C., the Peltier force for these metals is greater, being 4600 microvolts, and the calculated heat of combination is then $\frac{1}{3}$ of a unit per gramme of each, sufficient to raise the whole mass of metal through nearly 2 degrees Centigrade. This, then, is the sort of elevation of temperature one may expect in making brass at a temperature of 400° .

Take a gramme-equivalent of any metal, say 65 grammes of zinc, and imagine it rolled out into a thin sheet of foil of area A . The difference of potential between it and the air being V in electrostatic units, $\frac{V}{300}$ in volts, its charge will be $\frac{2AV}{4\pi x}$, where x is the distance between it and the air, a quantity of molecular magnitude. The electrical energy of this charge is $\frac{AV^2}{2\pi x}$, which must therefore have been the electrical work done (*i. e.* the amount of potential chemical energy transmuted into electrostatic energy) in spreading out the zinc over so much surface. [Capillary tension is part of the *mechanical* work done.]

Now let it be rolled so thin that every atom of it is in contact with air, *i. e.* let its thickness be also of molecular magnitude x . We can regard its potential energy in two ways: either as chemical or as electrical. Chemically its energy, measured by heat of combination, is

$$46,000 \text{ VJ},$$

where V is expressed in volts. Electrostatically its energy is

$$\frac{A}{2\pi x} \cdot \left(\frac{V}{300}\right)^2.$$

Equating these two values, and writing for the quantity of metal $m = Ax\rho$, we have the general relation

$$mV = .828 \pi \rho x^2 J \times 10^{10};$$

whence, taking $m = 65$, $\rho = 7$, and $V = 1.8$, we get, as our estimate of linear molecular dimensions,

$$x = 4 \times 10^{-9}.$$

The data in this calculation are all very definite; hence if the reasoning is legitimate, this estimate ought to be a pretty good one. It is true that another metal would give a rather different estimate, unless $\frac{mV}{\rho}$ were constant for all; but for ordinary metals—*e. g.* zinc, iron, copper, mercury, silver—this is not so outrageously far from being the case; though discrepancies arise with such metals as sodium on the one hand, and platinum on the other. But it is very doubtful whether platinum could be regarded as an oxide, however thin it were beaten; and sodium would probably take fire long before the proper molecular thinness was reached.

The several estimates of Sir William Thomson for the size of atoms were given in 'Nature,' March 1870, and are reproduced

in Thomson and Tait, Part II. Appendix F. In a lecture on the size of atoms delivered at the Royal Institution in February 1883 he restates these estimates with slight modifications thus:—

If atomic dimensions are comparable with 10^{-8} centim., brass would rise 62° C. at the instant of formation; while if atoms are so small as 2.5×10^{-9} , it would rise 1000° C. Hence 10^{-8} is to be regarded as a limit of smallness.

A soap-film so thin as 10^{-8} centim. would raise itself 280° by collapsing; therefore there are not several molecules in this thickness.

The theory of gaseous collision, combined with the density of liquids, suggests a range lying between 7×10^{-9} and 2×10^{-9} .

The dispersion of light seems to require atomic dimensions to lie between 10^{-7} and 10^{-9} .

The final estimate made by Sir William is something between

$$2 \times 10^{-7} \text{ and } 10^{-9}.$$

But if the reasoning in the present paper be admitted as correct, it would seem possible to reduce this range of uncertainty, and to make an even more precise estimate.

XXXIX. *Lecture-Experiments on Spectrum Analysis.*

By E. CLEMINSHAW, M.A., F.C.S.*

I PROPOSE in the following paper to explain some processes by which all the phenomena in spectrum analysis, usually shown upon the screen in class-demonstrations, may be exhibited without the use of the electric light, which for various reasons is frequently not available. The methods employed depend upon the use of the limelight and the oxy-hydrogen flame: these of course do not give the same brilliant effects as the electric light, but all the phenomena may be shown upon the screen with much simpler apparatus; and for some of my methods I think I may claim advantages over the usual method of demonstration.

i. *Spectra of the Alkalies and Alkaline Earths.*

Debray (*Ann. Ch. Phys.* [3] lxx. p. 331) proposed the use of the oxy-hydrogen flame for the volatilization of the alkalies and alkaline earths. By a modification of the ingenious method of Bunsen for procuring a monochromatic flame, the sub-

* Communicated by the Physical Society: read March 14, 1885.

stances in question may be conveniently introduced into the flame. Bunsen made hydrogen in a bottle containing a strong solution of sodium chloride with zinc and hydrogen sulphate, passed coal-gas into the bottle in order to increase the size of the flame, and to assist in the mechanical carrying-over of the spirtings of the sodium-chloride solution, and burnt the mixed gases in a description of Bunsen burner.

Instead of burning the mixed gases in air, I propose to burn them in an atmosphere of oxygen, by which I find a sufficiently bright flame can be obtained to show the spectra on the screen.

The mixed gases are burnt from a jet consisting of two concentric brass tubes, the inner of which passes into the gas-generating apparatus, and the outer is connected with a supply of oxygen; the inner tube is from $\frac{1}{8}$ – $\frac{3}{16}$ inch diameter, and may with advantage be wider at the bottom. A better result is obtained by passing hydrogen into the bottle, since it is not easy to render the coal-gas perfectly non-luminous by the oxygen.

A rapid evolution of hydrogen is necessary for a good result. The lens or lenses should be arranged so as to obtain as bright an image of the slit as possible, even at the expense of some spherical aberration.

The light obtained with sodium chloride is perfectly monochromatic and very bright. I venture to recommend this method of obtaining a bright monochromatic flame for lecture-demonstrations.

With a strong solution of lithium chloride the red band (Li α) can be shown, and on adding some strong solution of sodium chloride the yellow band of sodium.

For showing the spectra of the alkaline earths I use a saturated solution of the chlorides and generate hydrogen with zinc and hydrogen chloride. With two CS₂-prism bottles a red, orange (Sr α), and blue (Sr δ) can be shown on the screen; and, if the slit is narrow, the yellow band of sodium is clearly shown as soon as some sodium-chloride solution is introduced into the gas-generator. If a strong solution of lithium, sodium, and calcium chlorides are used, with two CS₂-prisms five bands can be shown on the screen, including the orange, green, and violet bands of Ca.

The above method is applicable for all compounds which can be volatilized in the oxy-hydrogen flame, and which are not reduced by the nascent hydrogen.

If hydrogen gas is passed into the bottle, or sufficient oxygen used to destroy the luminosity of the coal-gas, no trace of a continuous spectrum can be seen. An occasional addition

of acid, to maintain the evolution of hydrogen, keeps the flame burning brilliantly for some time ; this method might, therefore, be useful for spectroscopic observations for comparison.

ii. *Continuous Spectrum showing Bright Bands.*

If the slit is narrow, the orange and green bands of calcium may often be seen on the spectrum obtained with the lime-light. If the chlorides of the alkalis or alkaline earths are previously melted upon the surface of the lime-cylinder, the bands of these compounds become very bright. Four different compounds might be melted upon different portions, and the spectra of each shown in turn by turning the cylinder. The lime-cylinder should be a "hardlime;" the softer cylinders are too porous.

iii. *Reversed Spectra.*

The important fact that the light emitted by incandescent sodium vapour is opaque to light from the same source, is easily shown in a striking manner upon the screen by placing the apparatus used for showing the yellow band of sodium in § i. in the centre of an ordinary optical lantern ; a bright disk of monochromatic sodium-light may be obtained on the screen from 4-5 feet in diameter. A small Bunsen burner is then placed as near as possible to the condensing lens, with the top of the tube projecting $\frac{1}{2}$ inch above the edge of the lens, which shows the shadow of the tube at the top of the disk upon the screen. When a platinum wire containing a quantity of sodium chloride is introduced into the flame of the Bunsen burner, the dark appearance of the cooler flame is clearly seen on the screen.

Reversal of D line.

Mr. Lewis Wright states that this can be done with the limelight by burning sodium in a Bunsen burner placed as close to the slit as possible. I have not been able to obtain very satisfactory results by this method ; the flame is too hot. I have been more successful with the following methods :—The sodium is burnt in a small spoon placed between the lens and prism, *just* below the focus of the rays from the lens, which must be made as small as possible, so that all the rays are made to pass through the flame where the sodium vapour is densest, and none of the light from the sodium vapour is focused on the screen, and a much smaller flame may be used. Or the spoon may be placed a little in front of or beyond the focus in the path of the rays ; a shadow of the spoon is seen

across the spectrum, and with a small flame a marked thickening of the dark line is seen just above the spoon. The sodium may be burnt in a Bunsen burner or in the flame of a spirit-lamp.

If the sodium-flame is placed between the slit and the lime-light, I have obtained a good result by using a Bunsen burner, the flame of which is cooled down to a proper temperature by a mixture of air and carbon dioxide. Care must be taken not to pass carbon dioxide into the flame to excess, otherwise too great a lowering of its temperature takes place. I have, for instance, obtained in this manner a bluish flame which did not show the slightest trace of sodium in a room where sufficient sodium had been burnt to make every gas-flame give a strong sodium reaction.

The supply of carbon dioxide to the Bunsen burner may be adjusted in the following manner :—A cork is attached to the movable cover which closes the two holes for admitting air, and two holes made in it opposite the air-holes ; to one a glass tube is attached which is connected with a bottle, into which carbon dioxide is passed, fitted with three openings. By opening clamps the carbon dioxide may be all passed into the Bunsen burner, or passed directly out from the bottle. The sodium is first brought into vivid combustion, and then the flame cooled down by admission of the proper supply of carbon dioxide.

The reversed line may also be shown by burning sodium in a spirit-lamp with four wicks, in the centre of which is a jet for the admission of oxygen. This is placed between the slit and the limelight. On passing oxygen into the flame, the heat may be raised sufficiently high to produce a bright Na band upon the screen, especially if the light from the incandescent lime is somewhat moderated, and turned into a dark band when the oxygen is shut off, proving that the production of a dark or bright sodium-band depends upon the temperature of the absorbent vapour.

XL. On the Characteristic Curves and Surfaces of Incandescence Lamps. By J. A. FLEMING, M.A., D.Sc. (Lond.), Fellow of St. John's College, Cambridge.*

RECENT issues of a scientific journal† have contained some interesting letters and notes on the life of incandescence lamps, and on resulting deductions to be made

* Communicated by the Physical Society : read March 14, 1885.

† 'The Electrician,' vol. xiv. (1885), pp. 246, 294, 311, 347.

therefrom. The difficulty of the full and complete discussion of this subject is the absence of sufficiently prolonged experiments to give statistics reliable for this purpose. These can only be obtained at great expense and by experiments lasting over a considerable time; but the results to hand make a preliminary investigation interesting on the connection which exists between the life of incandescence lamps and other correlated quantities.

The manufacture of incandescence lamps has now advanced to such a condition that the accidents of manufacture are greatly under control. The conditions necessary to get a good lamp are fairly well understood, and the physical actions going on in the lamp are also to a great extent known. We now know that the expectations of earlier investigators of getting an absolutely unalterable carbon incandescence lamp are not destined to be fulfilled; but we know that the gradual destruction of the filament is an operation dependent upon several causes, which may be greatly delayed by attention to, and success in, certain operations of manufacture.

The gradual destruction of the carbon filament in a vacuum lamp is a kind of erosion taking place at one or more points. Observation seems to show that in a single loop-filament this cutting through takes place most generally near the negative side. The determining cause of breakage is, however, temperature; and carbon filaments which present such inequalities of resistance as to give rise to spots of higher temperature might, other things being equal, be expected to be doomed to a short career. Lamp-filaments are therefore like human lives: some come into the world with a taint of disease upon them, in the shape of irregularity of structure, which predisposes to an early death; but nevertheless, as even in the case of suicide, the great law of averages overrides particular instances, and gives us, in the case of a sufficiently extended series of observations, a law connecting the average behaviour under fixed circumstances.

Experience gained during three years of commercial manufacture and use of incandescence lamps has demonstrated that when a large number of filaments are prepared with identical care, and the lamps made with them sorted out into batches, and worked with varying electromotive force, there is a very constant relation between the average efficiency or candles per horse-power and the average duration or life, and the working potential or volts of the lamp. Now, in a general way we do not know what the form of the function is that connects these three quantities, or any two of them, but there have been a large number of observations on the relation of

certain variables which indicate as a most probable form an exponential function.

There are four variables between which a relation is required—electromotive force, resistance, candle-power, and life. The third of these is at present somewhat vague and indeterminate. What we really are concerned with is the total eye-affecting radiation; and our present methods only allow of a certain more or less imperfect comparison of this as a whole with that of a standard candle, or a calculation of the integral deduced from observation of the relative intensities of certain rays. Accordingly, of these four quantities two alone can be measured with any great accuracy. One is merely an average, and the other is necessarily a somewhat ill-defined quantity.

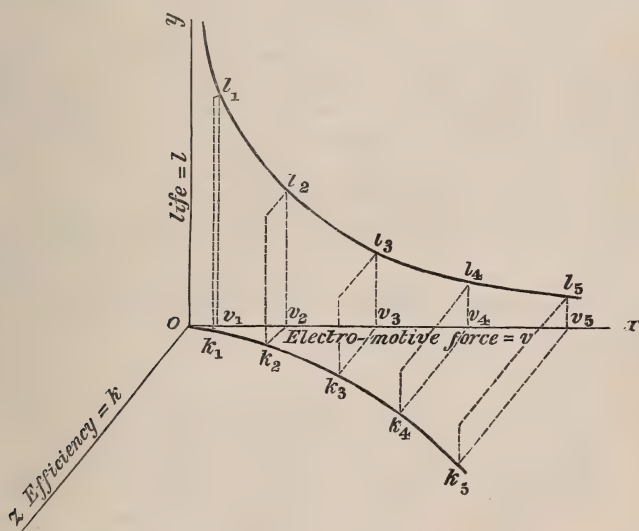
Between any two of these variables we can seek a relation, and having a number of observations we plot down these in what are best called the characteristic curves of the lamp. Now two of the most important of these curves are those connecting the electromotive force and efficiency, or candles per horse-power, and the electromotive force and life; and they may therefore be called the principal characteristic curves of the lamp. Three other useful curves may be obtained by plotting down the curves connecting candle-power and current, candle-power and electromotive force, and electromotive force and resistance. These may be called subsidiary characteristic curves.

Since the life and efficiency of a lamp vary together with the electromotive force, we can only properly represent the relation between the three by a *surface*, which may be called the characteristic surface of the lamp.

Take three rectangular axes, x, y, z (fig. 1), and let distances measured outwards represent life, candles per horse-power, and electromotive force. Let a curve be drawn on the yx plane, representing the relation of electromotive force and life, and one on the xz plane representing electromotive force and efficiency; let the ordinates be drawn at various points to both these curves, starting from abscissæ, representing certain pressures v_1, v_2 , &c. Complete the rectangles on the ordinates k, l , &c.; and we see that these rectangles form the orthogonal sections of a solid bounded respectively by the two planes xy and xz , and two curved surfaces, of which the characteristic curves l and k are the traces on these planes. The surface of this volume may be called the characteristic surface of the lamp. We see that the area of the orthogonal section parallel to yz gradually increases to a maximum, and then decreases. This is obviously because, for zero electromotive force, life is infinite and candles per horse-power zero;

whilst for very high electromotive force, life is zero and candles per horse-power or efficiency a maximum. Now the area of cross section which is a maximum for a certain value v_3 of electromotive force represents the product of life and candles per horse-power, or the maximum candle-hours per horse-power it is possible to get; and the value of kl is therefore a very important quantity. I shall call this maximum value of kl the principal modulus of the lamp, because the value of the lamp for commercial purposes is obtained by dividing the numeric representing this principal modulus by the price of the lamp, taking either the cost of manufacture or the selling-price, according as the question is considered from a manufacturer's or purchaser's point of view.

Fig. 1. Diagram of the principal Characteristic Curves of a Lamp.



The product kl is itself a function of the electromotive force, and the value of the electromotive force which makes this quantity a maximum is an important one to determine. It is the pressure at which the lamp should be worked in order to realize the greatest quantity of light for a given expenditure of energy in the lamp.

Let us next consider the form of the function which expresses these characteristic curves. Take, for instance, the curve of life and electromotive force. We do not know whether this curve is a continuous curve, whether it is asymptotic to axis of y , or, in fact, how it behaves beyond

the limits of the values of electromotive force v , for which lamps are incandescent. We have, however, certain values for l corresponding to values of v not lying far on either side of the ordinary so-called "marked volts" of the lamp. When the writer was in America he was shown a large number of observations, made with the view of determining the relation between average life and electromotive force, not far on either side of 100 volts, taken for the Edison 108-volt A lamp. For this lamp the life-pressure curve at or near the working-pressure is approximately a logarithmic curve whose equation is

$$l = Av^{-\alpha},$$

where A and α are constants; and for the Edison 16-candle 105-volt lamp α is nearly 25; and, accordingly, life varies inversely as the 25th power, roughly, of the electromotive force. In the figures shown to the writer the life-pressure curve had been drawn for pressures corresponding to lives far in excess of what could actually have been observed during the time filament-lamps have been made, reaching up to 11,793 hours. This is obviously improper: we do not know that α is not itself a function of v , and for pressures departing far from the ordinary working-pressure the variation of average life and pressure is not probably expressed by so simple a relation; and we cannot go fairly beyond the limits of actually observed lives. M. Foussat has recently communicated to a scientific journal the results of observations on the life of French Edison lamps.

TABLE I.

Volts.	Life.	\log_{10} (volts).	\log_{10} (life).
95	3595	1.97772	3.55570
96	2751	1.98227	3.43949
97	2135	1.98677	3.32940
98	1645	1.99123	3.21617
99	1277	1.99564	3.10619
100	1000	2.00000	3.00000
101	785	2.00432	2.89487
102	601	2.00860	2.77887
103	477	2.01284	2.67852
104	375	2.01703	2.57403
105	284	2.02119	2.45332

Now the figures given by M. G. Foussat on p. 246 of 'The Electrician' for 1885, stated to be the result of a large number of observations on the relation of average life and pressure at or near 100 volts, conform approximately to the above law. Taking M. Foussat's numbers for life and electromotive force,

and taking logarithms of both, we have the figures given in Table I.

Writing down the differences between successive values of $\log v$ and $\log l$, we get the numbers—

455	11621	432	10513
450	11009	428	11600
446	11323	424	10035
441	10998	419	10449
436	10619	416	12071

Now, if $l = Av^{-\alpha}$, then

$$\log l - \log l_1 = -\alpha(\log Av - \log Av_1),$$

where l and l_1 , v and v_1 are adjacent values of l and v ; accordingly,

$$\alpha = - \frac{\log l - \log l_1}{\log Av - \log Av_1}.$$

The mean of the differences of $\log l$ divided by the mean of the differences of $\log v$ gives 25.4 nearly; and, accordingly, M. Foussat's numbers agree with those found in America in assigning to α a value not far from 25 for the Edison lamp. In the neighbourhood of 100 volts

$$l = Av^{-25};$$

or, average life varies inversely as the 25th power of electromotive force.

This, however, is easily seen to be a very rough approximation. The successive quotients of life-difference by volt-difference are not constant; and the above simple exponential formula cannot be admitted as anything more than a very imperfect connection. In examining the Carlisle Tables of Mortality, which give the expectation of life at every age drawn from a very large number of observations, it was apparent that an empirical formula connecting the two quantities could be obtained of a form

$$\log e = a + bx + cx^2 + \&c.*,$$

where e = expectation of life at any age x .

I was led therefore to try if such an empirical formula better suited the present case; and a process of trial and

* Let x be the age, and e the expectation of life at that age. Then by the Carlisle Tables of Mortality at the several ages 10, 20, 30, 40, 50, 60, 70, 80, 90, the corresponding expectations of life are :—48.82, 41.46, 34.34, 27.61, 21.11, 14.34, 9.18, 5.51, 3.28; and it can easily be found that

$$10 \log e = 17.2 - \frac{x}{100} - \frac{14}{10,000} x^2, \text{ very nearly;}$$

the calculated values from which are :—49.6, 44, 36.6, 28.6, 20.9, 14.3, 9.64, 5.54, 3.14.

failure showed that

$$10 \log l = 135 - v - \frac{v^2}{2000}$$

is a formula which gives very nearly correct results in calculating the life of a lamp l , given the working-pressure v in volts. This may be expressed otherwise:—

$$\log l = 13.5 - \frac{v}{10} - \frac{v^2}{20,000},$$

or

$$l = 10^{13.5 - .1v - .00005v^2}.$$

Calculating by this formula, we get the following values for $\log l$ and l , l being the average life in hours of the 100-volt Edison lamp as made in France:—

TABLE II.

Calculated by formula.		Observed.	
l .	$\log l$.	$\log l$.	l .
3539	3.5488	3.5557	3595
2749	3.4392	3.4395	2751
2136	3.3296	3.3294	2135
1658	3.2196	3.2167	1645
1289	3.1100	3.1062	1277
1000	3.0000	3.0000	1000
776	2.8900	2.8948	785
602	2.7798	2.7788	601
467	2.6698	2.6785	477
362	2.5592	2.5740	375
281	2.4488	2.4533	284

It is evident, then, that the simple exponential function does not give nearly so good results as a formula of this latter description; and there is no doubt but that by a suitable selection of constants a formula can be obtained expressing the life of the lamp as a function of electromotive force throughout an observed range, which shall be closely in accordance with observed facts*. Until, however, a much

* Mr. F. M. Wright has given, in 'The Electrician,' p. 311 (1885), a formula—

$$100 - V = 9.098213 \log \frac{L}{1000}.$$

This is equivalent to $9.09 \log L = 127 - V$;

and Professors Ayrton and Perry have given

$$L = 10^{14 - .11v}$$

or

$$10 \log L = 140 - 1.1v,$$

both of which are nearly equivalent to the formula given in the text.

larger collection of statistics is obtained we shall not be in any position to determine if these constants are definite for each type of lamp or kind of carbon, and whether the average life of a lamp can be predicted from a knowledge of these constants.

An attempt was made, in the next place, to endeavour to obtain an approximate empirical formula connecting the efficiency of a lamp, or the candles per horse-power, and the electromotive force. On April 13, 1882, Prof. A. Jamieson read a paper before the Society of Telegraph Engineers and of Electricians (*Journ. Soc. Tel. Eng.* vol. xi. p. 164), "On Tests of Incandescent Lamps," and he has there given a number of tables and curves for different lamps, giving the efficiencies and resistances for various electromotive forces.

These observations afford a convenient means of putting to the test empirical formulæ, because the observations seem to have been carried out with very great care, and being done with secondary and primary batteries as current generators, the observations are more likely to be accurate than when a current from a dynamo machine is used; also because the observations for candle-power were entrusted to Dr. Wallace, gas-analyst for Glasgow, and were therefore in the hands of an observer whose eye was probably more trained to detect minute differences of illumination than one not so familiar with such work.

Professor Jamieson gives one complete table of the constants of an Edison 8-candle lamp over a great range of candle-power. Selecting that portion of the table in which the electromotive force was high enough to illuminate the lamp, we have as follows:—

TABLE III.

Tests of an Edison 8-candle Lamp, made by Prof. Jamieson, March 8, 1882.

Resistance, R.	E.M.F. in volts, V.	Current, in amperes, A.	Candle-power, K.
63·7	45·9	0·722	5·2
63·3	46·7	0·737	6·2
62·7	48·3	0·77	8·2
61	51·9	0·85	12·9
60·6	53	0·874	14·3
59·3	56·2	0·948	21·3
58·4	58	0·995	25·3
57·8	61·1	1·06	35·8
57	63·1	1·21	43·8

If we take the logarithms of these numbers we have the following table:—

Edison 8-candle lamp.

log R.	log V.	log A.	log K.
1·80414	1·66181	1·85854	0·71600
1·80140	1·66932	1·86747	0·79239
1·79729	1·68395	1·88649	0·91381
1·78533	1·71517	1·92942	1·11059
1·78247	1·72428	1·94151	1·15534
1·77305	1·74974	1·97681	1·32838
1·76641	1·76343	1·99782	1·40312
1·76193	1·78604	0·02531	1·55388
1·75587	1·80003	0·08279	1·64147

Let E be the watts of the lamp and k the efficiency or candles per horse-power. Then

$$k = \frac{K 746}{AV};$$

and if we calculate the logarithms of the efficiency corresponding to each electromotive-force value, and compare these with four times the value of the corresponding electromotive force, we have the following table:—

log k .	4 log v .	4 log v - log k .
2·06839	6·64724	4·57885
2·13834	6·67728	4·53894
2·21611	6·73580	4·51969
2·33879	6·86068	4·52189
2·36229	6·89712	4·53483
2·47457	6·99896	4·52439
2·51461	7·05372	4·53911
2·61527	7·14416	4·52889
2·63137	7·20012	4·56885
		Mean = 4·52968 = log 33860

Excepting the first and last values, which are the result of observations on the candle-power at extreme values, the intermediate figures are not very far from constant, and indicate, as a first rough approximation, that efficiency varies as the fourth power of the electromotive force.

At both high and low candle-powers the comparison of the light with a standard candle is difficult. In one case an excess of red, and in the other an excess of violet rays makes the comparison of the naked lights much more difficult at extremes, and, as is well known, the efficiency for very high or low candle-power must be stated for a definite radiation. The only other comparison between these two variables

attempted has been in the case of the experiments made by the Committee appointed to report on the incandescence lamps in the Paris Electrical Exhibition, an abstract of which appears in the same volume of the 'Journal of the Society of Telegraph Engineers.' The Committee condense these results on four varieties of lamps into the following numbers for the efficiencies, or candles per horse-power and electromotive force working them, taken at two very different values.

	Edison.		Swan.	
	89.11	98.39	47.3	54.21
E.M.F.	196.4	307.2	177.9	262.5
Candles per horse-power ...				
	Lane-Fox.		Maxim.	
	43.63	48.22	56.49	62.27
E.M.F.	173.6	276.9	151.3	239.4
Candles per horse-power ...				

If we take the logarithm of each number, and compare the difference of the logarithm of the efficiencies with the difference of the logarithm of the corresponding electromotive forces for each lamp, we have the following ratios :—

$$\begin{array}{l} \text{Edison.} \\ 19428 \\ 4302 = 4.5, \end{array}$$

$$\begin{array}{l} \text{Swan.} \\ 16895 \\ 5922 = 2.9. \end{array}$$

$$\begin{array}{l} \text{Lane-Fox.} \\ 20277 \\ 4344 = 4.6, \end{array}$$

$$\begin{array}{l} \text{Maxim.} \\ 19928 \\ 4231 = 4.7. \end{array}$$

If, therefore, k_1 and k_2 be the efficiencies corresponding to two observed values v_1 and v_2 of electromotive force,

$$\frac{\log k_1 - \log k_2}{\log v_1 - \log v_2} = 4.5 ;$$

or $k = Cv^{4.5}$

represents an approximate formula for calculating the efficiency. If, now, approximately, in the case of an Edison lamp, the candles per horse-power vary as the 4.5 power of the electromotive force, and the life varies inversely as the 25th power of the electromotive force, it follows that

$$l \propto \frac{1}{k^{5.7}}$$

or, roughly, that the life varies inversely as some power between 5 and 6 of the candles per horse-power.

The writer was given in America an extensive series of observations on the relation between the candles per horse-power and the average life, which were as follows :—

Life.	Candles per H. P.
11793	100
7150	110
4528	120
2974	130
2015	140
1403	150
1000	160
727	170
539	180
406	190
310	200
236	210
102	220

The length of life, 11,793 hours, given as corresponding to an efficiency of 100 candles per horse-power, is nearly equal to four years of average burning in working hours, and could not have been the result of actual observation; but these numbers agree very closely with the law that

$$l \propto \frac{1}{k^{5.25}},$$

where l represents life in hours and k candles per horse-power. It is most probable that this law has been deduced from observations on the life lying within an observed range and then extended by calculation to efficiencies below those actually observed. In any case these observations are not in great discord with the above deductions made from the numbers furnished by M. Foussat on the connection between life and electromotive force, in conjunction with other observations on the relation of efficiency to working pressure in the case of Edison lamps.

It is, however, far more probable that the connection between the candles per horse-power and the working pressure is expressible by a formula of this kind,

$$\log k = \alpha + \beta v + \gamma v^2 + \&c.,$$

where α, β, γ are known constants; and in this case we should have then both the life-pressure characteristic curve, and the efficiency-pressure characteristic curve expressed by analogous equations:—

$$\log l = a + bv + cv^2 + \&c.,$$

$$\log k = \alpha + \beta v + \gamma v^2 + \&c.$$

We have seen above that such a formula does fit in with observed values for one pair of variables. Further examination of this point is desirable.

Professors Ayrton and Perry have drawn attention * to a connection between the candle-power of a lamp and the working potential, and they find that in very many cases the cube root of the candle-power is proportional to the working potential minus a constant. Now, this is equivalent to saying that the candle-power of the lamp is proportional to the cube of the potential measured above a certain point.

I have examined some records of measurements of Edison lamps to put this law to further test, and the results are given below for two 16-candle Edison lamps measured by myself, and one 8-candle lamp measured by Prof. Jamieson.

Edison 16-candle Lamp.—No. 1.

Candle-power = K.	Volts, v .	$\sqrt[3]{K}$.	$v - 57.17$.	$\frac{v - 57.17}{\sqrt[3]{K}}$.
16	105.27	2.5726	48.10	18.8
11.5	99.56	2.2572	42.39	18.8
8.25	94.32	2.0206	37.15	18.4
4.8	90.03	1.6869	32.86	19
2.8	85.74	1.4095	28.57	20
2	81.22	1.2599	24.05	19.08

Edison 16-candle Lamp.—No. 2.

K.	v .	$\sqrt[3]{K}$.	$v - 50$.	$\frac{v - 50}{\sqrt[3]{K}}$.
16	103.84	2.5198	53.84	21.5
13	100.03	2.3513	50	21.3
9.5	97.65	2.118	47.65	22.4
8.25	94.79	2.019	44.79	22.1
7	92.89	1.913	42.89	22.3
5.5	88.12	1.765	38.12	22.2

Edison 8-candle Lamp used by Professor Jamieson.

K.	v .	$v - 28.7$.	$\sqrt[3]{K}$.	$\frac{v - 28.7}{\sqrt[3]{K}}$.
5.2	45.9	17.2	1.7324	9.928
6.2	46.7	18	1.8371	9.798
8.2	48.3	19.6	2.0165	9.720
12.9	51.9	23.2	2.3453	9.892
14.3	53	24.3	2.446	9.937
21.3	56.2	27.5	2.772	9.921
25.3	58	29.3	2.936	9.981
35.8	61.1	32.4	3.296	9.831
43.8	63.1	34.4	3.525	9.759

* "On the most Economical Potential-Difference to Employ with Incandescent Lamps: " 'The Electrician,' March 7, 1885, p. 348.

In these cases we see that the cube root of the candle-power is very nearly proportional to the excess of the electromotive force above a certain point. Now, on examining a number of such cases, it appears that this constant is the value of that electromotive force at which the lamp just begins to give signs of incandescence. In the case of the 8-candle lamp above, Professor Jamieson marks in his table against the E.M.F. 28·7 "filament bright red." It will be very interesting if further examination should give confirmation to this surmise; but the above figures seem to indicate that the cube root of the candle-power is proportional to the electromotive force reckoned from the neighbourhood of that pressure at which the filament begins to give out light. If we call this excess pressure the "effective volts," then we can state the rule that the cube root of the candle-power is proportional to the effective volts. We have then, according to the observations of Professors Ayton and Perry, in a certain number of cases an empirical law of this kind,

$$\sqrt[3]{K} = a(v-b),$$

in which K is the candle-power measured at a pressure v .

This may be written

$$\frac{1}{3} \log K = \log a + \log (v-b).$$

By ordinary algebraic theory we have

$$\log a = \overline{a-1} - \frac{1}{2}\overline{a-1}^2 + \frac{1}{3}\overline{a-1}^3 - \&c.;$$

\therefore if $c=b+1$, we can write $\log (v-b)$ as equal to the series

$$\overline{v-c} - \frac{1}{2}\overline{v-c}^2 + \frac{1}{3}\overline{v-c}^3 + \&c.,$$

and

$$\log K = A + B\overline{v-c} + C\overline{v-c}^2 + D\overline{v-c}^3 + \&c.$$

If the supposition above made is correct, that b is a value not far from that pressure at which the lamp becomes incandescent, then k is seen to be an exponential function of the effective volts of a kind similar to that which has been found to reconcile very well the values of observed life and corresponding working electromotive force.

Other observers have before now called attention to the fact, that within a certain range the candle-power of a lamp varies approximately as the sixth power of the current passing*.

In the case of the above-mentioned Edison 16-candle lamp,

* At the British-Association Meeting at Montreal, Mr. Preece read a note on this subject, confirming previous observations made in 1883, and showing, from observations of his own, Professor Kittler, and Captain Abney, that incandescence varies very nearly as sixth power of current.

No. 1, the following values were obtained, connecting current and candle-power :—

Candle-power, K.	$\sqrt[6]{K}$.	Current in amperes, a .	$\sqrt[6]{K \div a}$.
16	1.5874	.7582	2.09
11.5	1.5024	.7065	2.12
8.25	1.4215	.6720	2.11
4.8	1.2988	.6203	2.09
2.8	1.1872	.5859	2.03
2	1.1225	.5514	2.02

The table shows that, with considerable accuracy, over a range from two to sixteen candles, the incandescence varies as the sixth power of the current.

In those characteristics into which candle-power enters in any way, there is a considerable difficulty in getting results with sufficient accuracy to determine the constants of the characteristic equations. There is, however, one pair of variables, namely electromotive force and resistance, both of which can be measured with very high accuracy over a great range; and some very interesting examples of these pressure-resistance curves are given by Professor Jamieson in his memoir above alluded to.

A very short examination of the way in which an incandescence lamp behaves under increasing electromotive force, shows that the resistance decreases with increase of electromotive force, but that it does not decrease without limit; it tends to a minimum value, beyond which it appears to be constant. This is very strikingly shown for some of the Swan lamps tested by Professor Jamieson. In his paper certain formulæ are given connecting various lamp-constants, and as a first approximation to a pressure-resistance equation is given the following :— $\log P = \log E + ar$,

where P is a constant, $E = \text{E.M.F.}$, and $r = \text{resistance}$.

It is not possible that such a formula should represent correctly the relation of resistance to pressure at high pressures, because it in no way expresses the fact that resistance tends to a minimum with increasing electromotive force.

An empirical formula can, however, be obtained which will express this in the following way :—

Let R be the resistance of a lamp measured with any electromotive force, E , at the terminals. Let E_0 represent the electromotive force at which the lamp just becomes incandescent, and let R_0 be the corresponding resistance. Let r be the minimum resistance to which the lamp approximates, as

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E increases, until the filament breaks. Then

$$R = r + 10^{\log(R_0 - r) - A(E - E_0)}$$

is an expression which has the following properties:—

If $E = E_0$, then $R = R_0$;

if $E = \infty$, „ $R = r$.

The above formula is otherwise written

$$\log(R - r) = \log(R_0 - r) - A(E - E_0).$$

This formula has been tested for an Edison 8-candle lamp of which the resistance was measured over a considerable range by Professor Jamieson, and the resistance-pressure curve given in his paper. For this lamp luminosity commenced at 28·7 volts, and at this pressure it had a resistance of 73·4 ohms; its resistance gradually decreased with increasing pressure until it became apparently constant and equal to 53·5 ohms. Taking A equal to $\frac{1}{50}$, we have

$$50 \log(R - 53\cdot5) + (E - 28\cdot7) = 50 \log(73\cdot4 - 53\cdot5) = 64\cdot945.$$

Calculating R by this formula for various values of electromotive force E, we have the following table of observed and calculated resistances:—

Edison 8-candle Lamp.

E.M.F.	Resistance observed.	Resistance calculated by above formula.	E.M.F.	Resistance observed.	Resistance calculated by above formula.
28·7	73·4	73·4	48·3	62·7	61·57
32·7	71·1	70·05	51·9	61	60·34
36·1	68·1	67·65	53	60·6	60
39·7	66·4	65·49	56·2	59·3	59·13
43·1	64·7	63·78	58	58·4	58·66
45·9	63·7	62·51	61·1	57·8	57·98
46·7	63·3	62·19	63·1	57	57·58

The accordance between the observed and calculated resistances is fairly close. It seems very probable, from an inspection of the values at very low electromotive forces, that the resistance is a function of the electromotive force, reckoned from the pressure b , at which the lamp-filament begins to be bright red, and that of R is the resistance corresponding to any electromotive force, E, and r is the minimum resistance to which the lamp tends. Then

$$\log(R - r) = A + B(E - b) + C(E - b)^2 + \&c.,$$

where A B C, &c. are constants. If this should be the case, then it may be possible to express the life, candles per horse-power, candle-power, and resistance of a lamp, all as similar functions of the electromotive force, knowing certain constants

and the two values of electromotive force at which the lamp becomes incandescent, and the resistance to which it finally tends to obtain. When, however, we are dealing with comparatively small variations of electromotive force, it is possible to calculate both life and efficiency from a simpler exponential function of the form—

$$l \propto v^x$$

$$R \propto v^y$$

when x and y are numerics. Assuming such an approximate formula, it becomes possible to deal with an interesting question, which has been discussed also by Professors Ayrton and Perry*, but which is capable of being investigated in a slightly different manner from that adopted by them in their paper. We shall take the warrant we have in the above figures for the assumption that for electromotive forces not far from those at which the lamp is intended to be used in the case of an Edison lamp, say 100 volts,

Average life varies inversely as the twenty-fifth power of the electromotive force ;

Efficiency, or candles per horse-power, varies as the fourth power of the electromotive force ;

Candle's light varies as the sixth power of the current, and therefore as the sixth power of the electromotive force, seeing that the resistance alters very little after the lamp has reached fair incandescence ;

And that therefore life varies inversely as the sixth-and-a-quarter power of the efficiency, and also inversely as the fourth-and-a-quarter power of the candle-power.

Let p be the price of a lamp in pounds sterling, or fractions of a pound.

Let l be the average life in hours, c the actual candle-power, and k the candles per horse-power, when run at a certain electromotive force v .

Then $\frac{p}{cl}$ is the cost of one candle-light per hour as far as the lamp itself is concerned. Let P be the cost of 1 horse-power hour of electric energy expended in the filament. P will not be the same for all amounts, 1000 P costs less than ten times 100 P , but for the small variations we are considering we shall consider P to be a constant. Then $\frac{P}{k}$ is the cost of the power in making one candle-light for one hour, and the total cost of getting one candle-light for one hour is

$$\frac{p}{cl} + \frac{P}{k} = T.$$

* "On Potential-difference, &c."

The first term is the expense of the translating device, and it may be called the cost of *lampage*.

The second term is the cost of the production of the energy which passes through the translating device or lamp-filament and is converted into eye-affecting radiant energy, and this may be called the *machinage*; hence the total expense of keeping going incandescent light is made up of lampage and machinage. Now, we can procure a given amount of light either by running the lamps very high, in which case lamps will cost a great deal, and power less in proportion, or we can run the lamps very low and save in lampage, whilst expending more in power in a greater number of lamps; and the question arises, apart from capital expenditure, at what point is the greatest economy obtained, or, in other words, what proportion ought lampage to bear to machinage in order that the total cost may be a minimum? To solve this we shall assume, as is very probable, that for the limit of variation of E.M.F. employed the average life of the lamp is an exponential function either of the candles per horse-power k , or of the candle's light c , for the particular lamps considered.

Let $l = \frac{A}{k^\alpha}$ and $l = \frac{B}{c^\beta}$ be the functions. α and β have definite values at a given E.M.F.; A and B are constants.

Then $cl = B'l^{\frac{1}{\beta}}$; $k = A'l^{-\frac{1}{\alpha}}$.

Substituting, we have $B'pl^{\frac{1}{\beta}-1} + A'Pl^{\frac{1}{\alpha}} = T$;

in which B' and A' are constants.

Now this expresses the total cost of working as a function of the average life at a certain E.M.F.

Let us also take that $l = \frac{1}{v^\gamma}$, where v is the E.M.F. at which the lamps are being run.

Then

$$B'p\left(\frac{1}{v^\gamma}\right)^{\frac{1-\beta}{\beta}} + A'P\left(\frac{1}{v^\gamma}\right)^{\frac{1}{\alpha}} = T,$$

or

$$B'pv^{-\frac{1-\beta}{\beta}\gamma} + A'Pv^{-\frac{\gamma}{\alpha}} = T.$$

Let this be varied by varying v ; to find at which value it becomes a minimum with respect to v , differentiate with respect to v , and equate to zero.

$$\frac{dT}{dv} = 0 = -\frac{B'\frac{1-\beta}{\beta}\gamma pv^{-\frac{1-\beta}{\beta}\gamma}}{v} - \frac{A'\frac{\gamma}{\alpha}Pv^{-\frac{\gamma}{\alpha}}}{v},$$

or

$$-\alpha\frac{1-\beta}{\beta}B'pv^{-\frac{1-\beta}{\beta}\gamma} = A'Pv^{-\frac{\gamma}{\alpha}}.$$

Hence the total cost of working is a minimum when the cost of power $A'Pv^{\frac{\gamma}{\alpha}}$ is to the lampage $B'pv^{-\frac{1-\beta}{\beta}\gamma}$ as $\alpha\frac{1+\beta}{\beta}$ to unity.

Now the above investigations show that for Edison lamps α is a quantity in the neighbourhood of $6\frac{1}{4}$, and β near $4\frac{1}{6}$; hence

$$-\alpha\frac{1-\beta}{\beta} = \frac{19}{4},$$

and hence ratio of lampage to total cost is $\frac{4}{23} = 17.4$ per cent.

Hence we arrive at this curious result, that independently of the cost of the lamp, or electrical energy, we must run at such a pressure that lampage is about 18 per cent. of the total cost. Now, if instead of employing these approximate exponential expressions for the values of l , c , and k in terms of the electromotive force, we had introduced the more accurate forms of equation indicated above, we should have had an equation in terms of v to solve as the result of equating the differential to zero, which would, by the introduction of the proper constants, give the value of electromotive force at which the total cost becomes a minimum. In their paper, Professors Ayrton and Perry have calculated to a fraction of a volt what this economical potential is. As, however, the characteristic equations are only approximate, it seems hardly necessary to do more than obtain a similar approximate expression for the economical working.

At the Edison lamp-factory in America calculations were made, on the assumption of a particular type of lamp and length of life and cost of power, to ascertain the ratio of lampage to total cost, which made the total cost a minimum, and the result appeared to be to fix it at about 16 per cent. These calculations are therefore in singular accord with the deduction of theory based on determination of the constants of the characteristic curves, arrived at both by graphic and analytical methods.

XLI. *Experiments on the Electromagnetic Action of Dielectric Polarization.* By Prof. W. C. RÖNTGEN*.

THE theory of electrical and magnetic phenomena proposed by Faraday and elaborated by Clerk-Maxwell, is based upon the assumption that in insulators bounded by electrified conductors there exists a dielectric polarization or displacement—a change which, in whatever way produced, exerts electrodynamic effects exactly like an electric current flowing in a conductor.

* Translated from the *Sitzungsberichte der Berliner Akad. der Wissenschaften* for February 26, 1885.

Helmholtz has shown that none of the consequences of this assumption are opposed to the fundamental laws of mechanics, and that this assumption, together with the extended law of potential, affords a complete explanation of the phenomena presented by closed and by so-called open conductors.

So far as I know, however, no direct experimental proof has been given of the correctness of the assumption of Faraday and Maxwell; and I have for several years had the intention to fill this gap when the opportunity presented itself. After many fruitless attempts I have at last been successful in finding a method which yields practical and decisive results, of which I venture here to give an account. A good insulating disk of ebonite, 0.5 centim. thick and 16 centim. diameter, was mounted upon a vertical axis, which by means of a string could be put into rapid rotation (120 to 150 revolutions per second) in a horizontal plane. Beneath the disk and parallel with it was placed a perforated glass plate of 17.5 centim. diameter, provided with two half-ring coatings of tinfoil: the inner radius of the rings was 2.25 centim. and the outer radius 7 centim.; the portion cut out between the two half-rings was 1.4 centim. wide. Above the ebonite disk also was fixed a second horizontal glass plate of 21.5 centim. diameter, which was completely coated with tinfoil. The tinfoil coatings of the two glass plates faced the ebonite disk, and were at a distance from it of about 0.1 centim.; the coating of the upper plate was permanently connected to earth. Either of the half-rings could be placed in connection with the inner coating of a large Leyden jar, so that the one became positively electrified, and the other at the same time negatively. A commutator permitted the electrification to be changed.

It will now be understood that the dielectric polarization produced in the rotating ebonite disk by the electrification of the tinfoil coatings changes its sign at the point where the interval between the coatings occurred. Upon the one half of the disk (say the front half) the particles moved from the positive towards the negative half-ring, and a displacement of positive electricity took place while they passed from the one half-ring to the other, which would have a vertical component tending downwards. At the same time on the other (hinder) half of the disk, there would be a vertical component tending upwards. These displacements lasted as long as the disk rotated with unaltered electrification; and they must therefore, according to the theory of Faraday and Maxwell, produce the same electromagnetic effect as continuous currents, which, for the direction of rotation assumed, would circulate downwards in the front half of the disk, and upwards in the

back half. The question is, then, whether these vertical components do actually exert such an effect.

In order to determine this, the upper glass plate was surmounted by a metallic case, connected to earth, which contained an extremely sensitive astatic pair of needles; the lower needle was about 0·6 centim. distant from the ebonite plate, its centre being in the prolongation of the axis of rotation of the disk, and its direction parallel to the dividing-line of the two half-rings; the length of the needle was about 4·8 centim., a little more than the inner diameter of the half-rings. The second needle was situated 21·5 centim. above the lower one. The deviations were read by means of a telescope and scale at a distance of 3 metres. All the necessary precautions were taken that the needle should not be affected by external statical electricity; and a special construction of the axis was adopted so that the deviations produced by rotational magnetism should be as small as possible (2 to 3 divisions of the scale). Notwithstanding, upon rapid rotation of the disk the needle changed its position of rest continually, which made the observations much more difficult. The reason of these motions lay, as I satisfied myself, in currents of air, and more particularly in small vibrations to which the apparatus was exposed, in consequence of the defective arrangements of the Institute here.

The experiments were arranged so that one observer sat at the telescope whilst an assistant revolved the disk, and another changed the commutator upon a signal from the observer.

The observer intentionally remained ignorant of the direction in which the commutator was moved until the end of the series of experiments; generally the commutator was changed eight times during each series of experiments. There was no possibility of accurately determining the magnitude of the deflection produced by reversal of the commutator, since in all cases it was very small and under the best conditions only amounted to 1·5 division of the scale, generally amounting only to a fraction of a division. The observer therefore confined his attention to determining the direction of the deflection each time, and for this a certain amount of practice was necessary in consequence of the continual small motions of the needle. As the result of more than 1000 observations I have obtained so much practice that in the later experiments I have been able almost without mistake to determine every time the direction of deflection.

From these experiments, which were varied in many ways, the result was obtained that the deflection always agreed with that given by Faraday's theory. The change in the dielectric

polarization consequently exerts an electromagnetic force, exactly like an electric current flowing through a conductor in the same direction in which the displacement of positive electricity in an insulator takes place.

The complete description of the experiments here briefly described, as well as of the numerous experiments made for the purpose of excluding all possible deception, will be given elsewhere.

I am now occupied with the construction of a piece of apparatus upon the same principle, which I hope will possess fewer defects and will be capable of producing greater deflections than those described. Also I intend to put to experimental proof some other of the consequences of Faraday's theory.

In conclusion, it should be mentioned that with the suitably arranged apparatus I have repeated Rowland's experiment described by Prof. Helmholtz as a test of its sensitiveness. The ebonite disk without coatings was charged by means of points. Upon reversal of the electrifications a deflection of 8 to 10 scale-divisions took place each time.

XLII. Notices respecting New Books.

Lenses and Systems of Lenses, treated after the manner of Gauss.

By CHARLES PENDLEBURY, M.A. Cambridge: Deighton, Bell, & Co.; 95 pp.

IN the ordinary treatment of a lens it has been usual in our textbooks to limit the discussion to the case of a "thin" lens only. The result is, a number of interesting propositions are obtained which are practically false. Gauss, as Mr. Pendlebury states, in a paper which he communicated to the Royal Society of Göttingen (Dec. 10, 1840), shows "how the solution of the (general) problem could be made to depend upon the determination, for each system and once for all, of four fixed points situated upon the axis of the system. These points having been determined, the complete solution became a matter of simple algebra or geometry." The method is applicable to any system of coaxial lenses, whatever their thicknesses, provided the angle made by any ray with the axis, and the distance from the axis of the point of section of any surface, are small. This is, so far as our reading extends, the first attempt to introduce the treatment to English readers in a work intended for students; for this the author deserves their thanks. We have noticed a very few passages where an idiom reminds us of a German origin; some few more where there are slips which may mislead the student; and some which appear to be incorrect. As, however, the author will no doubt have detected these and will remove them in the more extended treatment of the subject upon which we believe he is now engaged, we do not note them here.

On page 58, last two lines, t and ρ should change places. The chapters are:—Refraction at a Single Surface, at Two Surfaces in Succession, at any Number of Surfaces; Achromatism; Determination of the Foci and of the Principal Points, Nodal Points; Different Forms of Lenses; with an Appendix on Continued Fractions. There are no numerical examples given to exercise the student.

Transit Tables for 1885. By LATIMER CLARK, M.I.C.E., &c.
London: Spon, 1885; pp. 71.

MR. LATIMER CLARK continues his useful work by bringing out a new Edition of his Tables, which we understand are now ordinarily employed at Kew Observatory in place of those of the 'Nautical Almanac.' This year's issue differs slightly from its predecessors, the times of transit being given to the nearest tenth, instead of, as previously, to the nearest hundredth, of a second. There is also an additional set of Tables, giving for each alternate day certain *astronomical data*, among others, the Sun's semidiameter. The last would be much more conveniently placed immediately after the column containing the time of the Sun's transit. The introductory matter contains much useful information as to the method of using the Transit instrument and obtaining true time in any part of the world. The book will prove a very useful adjunct to the private observatory.

Electrical Units, their Relation to one another, and other Physical Units; with a Chapter on the Different Forms of Dynamos and a Series of Numerical Questions. By DR. R. WORMELL, M.A.
London: Murby; fcap. 8vo, pp. 48.

IN this, which is an Appendix to 'Magnetism and Electricity' by the same Author, there is a fairly lucid explanation of the various units in use in the science and of the manner in which they are connected. Several forms of Incandescent Lamps are described, as well as some of the Dynamos now most frequently used for the production of the Electric Light. The pamphlet concludes with a series of useful questions on Magnetism and Electricity generally.

XLIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 229.]

February 25, 1885.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S.,
President, in the Chair.

THE following communications were read:—

1. "On a Dredged Skull of *Ovibos moschatus*." By Prof. W. Boyd Dawkins, M.A., F.R.S., F.G.S.

2. "On Fulgurite from Mont Blanc." By Frank Rutley, Esq., F.G.S.

The specimens described in this paper were collected by Mr. J.

Eccles on the Dom du Gouté, about 14,000 feet above the sea-level. The rock, which has been fused by the lightning, is a hornblende gneiss. The fusion in the specimens examined is quite superficial. The hornblende has been converted into a dark, and the felspar into a white, glass, which, as a rule, remain distinct. The fulgurite in some cases consists of small spheres of glass, mostly of dark colour, which, in one instance, appear to have been spurted over the surface of the rock while in a state of fusion. The fulgurite glass is quite free from microliths, and shows only gas-bubbles and enclosures of glass, the latter usually containing nests of such bubbles. In conclusion, a comparison was made between this fulgurite and the Bouteillenstein or pseudo-chrysolite of Bohemia, which is now regarded by Makowsky and others as an artificially formed glass.

3. "On Brecciated Porfido-rosso-antico." By Frank Rutley, Esq., F.G.S.

The variety of this well-known hornblende-porphyrite here described shows a distinctly brecciated structure when examined in thin section under the microscope. The fragments sometimes appear to fit together, at others they are more or less widely separated, so that the section at first sight presents almost the aspect of a tuff. Careful examination shows that this brecciated structure is due merely to the rock having been crushed and the fragments connected *in situ* by siliceous infiltrations. Delesse's observations upon the varieties of the rock were discussed. A few additional remarks were also made upon the mineral constitution of the specimens described.

4. "Fossil Chilostomatous Bryozoa from Aldinga and the River-Murray Cliffs, South Australia." By Arthur Wm. Waters, Esq., F.G.S.

March 11.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "The Granitic and Schistose Rocks of Donegal and some other parts of Ireland." By C. Callaway, D.Sc., F.G.S.

The Author first recalled attention to the current theories on the nature of the Donegal granitic rock—one which described it as a highly metamorphosed portion of a sedimentary series, another which regarded it as a mass of Laurentian gneiss. In his view, however, it was a true igneous granite, posterior in age to the associated schists. In six districts examined it was intrusive and sent out veins. The apparent interstratification with bedded rocks was explained as a series of comparatively regular intrusions. Where the granite was seen in contact with limestone, the latter contained garnets and other accessory minerals. No gradation could be discovered between the granite and any other rock, the junctions (even in the case of small fragments of schist immersed in granite) being well marked.

The granite was distinctly foliated. In some localities there was merely a linear arrangement of the mica; but near the western

margin of the granite promontory there was a striping of light and dark bands, the colour of the latter being due to the abundance of black mica. The gneissic structure was attributed to lateral pressure, the existence of which in the associated strata was seen in the conversion of grits into schist-like rocks, in the production of cleavage in beds of coarse materials, in the crushed condition of some masses, in the overthrow of folds, and in the production of planes of thrust. The direction of the pressure was perpendicular to the planes of foliation in the granite.

The schistose rocks of the region were divided into two groups. The *Lough Foyle series* consisted of quartzites, quartzose grits with a mineralized matrix, slaty-looking schists, fine-grained satiny schists, black phyllites, and crystalline limestones and dolomites. The semicrystalline condition of most of these rocks was characteristic. This series was well seen at Londonderry and on Lough Foyle, and formed a broad band striking to the south-west. These rocks were compared with similar types in the Hill of Howth (north of Dublin), near Aughrim (co. Wicklow), and south of Wexford. The Leinster semicrystalline masses were quite unlike the Wicklow Cambrians, and bore a strong resemblance to the slaty series of Anglesey. They were lithologically intermediate between the Donegal and Anglesey groups, and from a comparison of all these areas the author referred the Lough-Foyle Series, with some confidence, to the Pebidian system. The prolongation of the Lough-Foyle rocks into the Grampian region was well known, and Ireland thus served to connect some parts of the Scottish highlands with South Britain. The author was not prepared to correlate this Donegal series with any American group; but the lithological affinities were rather with the Taconian than with the Huronian.

The *Kilmacrenan series*, in which the granite is intrusive, was described as crystalline and older than the Lough-Foyle group. It was mainly made up of micaceous, quartzose, hornblendic, and hydromagnesian schists, quartzites, and crystalline limestones. There were no indications in these rocks of a metamorphism progressive in the direction of the granite. This series was lithologically similar to the Montalban system.

Fifty-five microscopic slides of Donegal and Leinster rocks had been examined by Prof. Bonney, whose observations confirmed those of the Author both as regards the nature and relations of the granite and the general characters and state of crystallization of the two schistose groups.

2. "On Hollow Spherulites and their occurrence in ancient British Lavas." By Grenville A. J. Cole, Esq., F.G.S.

Many of the felstones of North Wales have been shown to be altered lava-flows of an originally glassy type. In several localities, as in the Pass of Llanberis, at the foot of the Glyder-fawr, these rocks contain numerous nodular bodies, from $\frac{1}{10}$ inch to some inches in diameter. The smaller varieties have the appearance of spherulites, but the larger are very often hollow, their cavities being partly filled with minerals deposited by infiltration. The lavas

thus receive a scoriaceous character, and have been described as slaggy and vesicular. Similar structures, such as the "Lithophysen" of Von Richthofen, occur in rocks of much later date, and the theory advanced by Szabó, that their cavities have been formed by the weathering-out of the centres of spherulites, has been pretty generally accepted. A consideration of hollow spherulites of very different sizes from Iceland, Lipari, and the Yellowstone area, tends strongly to support this view, those portions that show radial structure being most easily attacked by the agents of decomposition. The merely concentric coats of the spherulite, on the other hand, consisting mainly of globulitic particles and glass, remain but little altered, and a series of hollow shells may arise one within the other, by the complete removal of the intervening radial matter. The frequent association of perlitic structure and hollow spherulites in the same rock may be due to the number of channels provided in such cases for the passage of water or acid vapours.

Structures resembling the "Lithophysen" of Hungary occur in the altered rhyolites of the Wrekin, the cavities being filled with quartz; and the hollow nodules of the Silurian felsites in the Pass of Llanberis prove, on microscopic examination, to have been originally spherulites. Many of these nodules show marked radial structure in their central areas; and every gradation exists between the solid varieties and those which have been hollowed out or replaced by products of infiltration. The completion of such a process of alteration might cause a rock not originally vesicular to be regarded as an ordinary amygdaloid.

April 15.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., President,
in the Chair.

The following communications were read:—

1. "A General Section of the Bagshot Strata from Aldershot to Wokingham." By the Rev. A. Irving, B.Sc., B.A., F.G.S.

The Author referred to earlier papers in the 'Geological Magazine,' in which the green colouring-matter so common in the Middle and Lower Bagshot strata of the London Basin had been attributed to the presence of vegetable debris and the materials resulting from decomposition of vegetable matter. The marked difference in this respect between these strata and the higher members of the series furnishes a clue to the conditions under which they were respectively deposited, the former being delta- and lagoon-deposits, the latter the deposits of a marine estuary. This implies a transgressive overlap of the upper portions of the Bagshot series upon the London Clay; and the present paper was devoted to a consideration of the statistical evidence of this overlap.

Sections were described in detail at Aldershot, Farnborough, Yateley, Camberley, Wellington College and the neighbourhood, and from the last-named place to Wokingham. From these, a general section was constructed to exact scale, both as to thickness of strata and altitudes, showing a relation of the Bagshot formation to the London Clay, which was inconsistent with the generally received idea

of their conformability, and at variance with the mapping of the district as executed by the Geological Survey.

The importance of the Bagshot pebble-bed as a basement-line of the upper division of the Bagshot strata was shown, as was suggested by the author, so long ago as 1880.

The synclinal arrangement of the London Clay was shown to have been produced *before* the deposition of the Bagshot series, though a certain amount of movement (with a resultant amount of 150 feet of tilting in 13 miles from south to north) has since taken place.

2. "Notes on the Polyzoa and Foraminifera of the Cambridge Greensand." By G. R. Vine, Esq.

XLIV. *Intelligence and Miscellaneous Articles.*

EXPERIMENTAL RESEARCHES UPON THE DETERMINATION OF THE DIELECTRIC CONSTANT OF SOME GASES. BY DR. IGNAZ KLEMENČIČ, PRIVATDOCENT AND ASSISTANT IN THE UNIVERSITY OF GRAZ.

THE author determined the dielectric constant of some gases and vapours by the following method:—A large condenser, consisting of 30 nickel-plated brass circular plates of 25·76 centim. diameter, was put upon a plate and covered by a glass globe fitting air-tight to the plate. The plates of the condenser were separated from each other by small ebonite plates 0·89 millim. thick; but the rest of the space could be filled with air or any other desired gas. The density of this dielectric medium could be varied within certain limits, and the pressure read off upon a manometer. The condenser was placed in communication with an air-pump and with two large glass globes by means of tubes provided with taps. The condenser and glass globes could be exhausted to a tolerably low pressure, and it was also possible by means of a branch tube to fill the condenser with any gas at a high pressure. When it was necessary to exhaust the condenser rapidly, it was simply placed in communication with the exhausted glass globes.

This condenser was charged 64 times in every second, and discharged as frequently through a sensitive galvanometer by the use of a tuning-fork, from a powerful battery of 22 small Bunsen elements with solution of potassium bichromate. The deflection of the galvanometer-needle thus produced is, as is well known, a measure of the capacity of the condenser. If the electromotive force of the charging-battery and the frequency with which it is charged and discharged remain the same, any change in the capacity of the condenser produces a corresponding change in the deflection of the galvanometer.

If the dielectric constant of the medium varies with the density, then every change in the pressure within the condenser must produce a change in the position of the galvanometer-needle. The deflection produced in the present case by the discharge of the con-

denser was much too large to render possible the observation of such a difference in position: it was therefore necessary to compensate this deflection, *i. e.* by means of an equal opposite action upon the galvanometer-needle.

Of the various possible modes of compensation the author employed that of a constant current. A weaker battery (compensation battery of 4 Daniell cells) was employed to send a current through a large resistance, and through the coil of the galvanometer in the direction required to produce a deflection opposite to that given by the discharges of the condenser. By altering the large resistance of the compensation-current, it was easy to arrange that the needle should remain in its position of equilibrium under the simultaneous action of the two currents. If this adjustment had been made for a definite capacity of the condenser, and if then only the density of the dielectric medium was varied, a deflection of the galvanometer-needle was observed, due to the change in the capacity of the condenser produced by the change in density, and consequently in the dielectric constant of the medium. The ratio of the dielectric constants for two different densities of the medium may be calculated from the easily determined deflection produced by the compensating current and the observed deflection. In this calculation it is only necessary to take account of the circumstance that a small part of the condenser-discharge passes through the compensation-circuit, and not through the galvanometer. As is well known, both Professor Boltzmann and Messrs. Ayrton and Perry have employed the electrometer for their investigations on this subject. The condenser was charged for a much longer period than in the present case; and this constitutes an essential difference between the two methods. It is a great advantage of the galvanometric method that good insulation is by no means so necessary as with the electrometric method, and thus one of the chief difficulties which such measurements offer is avoided. The question whether the accuracy of the results is disturbed by electricity produced by the friction of the gas admitted to the condenser does not here arise.

Besides the seven gases already examined by Prof. Boltzmann, the author has also examined the dielectric constant of five vapours. The results obtained with the seven gases and the vapour of carbon disulphide agree very well with the determination of Boltzmann, and with the electromagnetic theory of light. The dielectric constants of the remaining vapours do not satisfy the conditions given by this theory. The deflection of the galvanometer-needle was observed by means of a telescope, mirror, and scale, and it should be noticed that a change of pressure of 690 millim. in atmospheric air caused a change in position of 185 scale-divisions. The results are given in the following table, in which D denotes the ratio of the two dielectric constants which the gas possessed at 0° C., and at two pressures differing by 760 millim.

The refractive indices n are taken from the determinations of Mascart. For the sake of comparison the corresponding results obtained by Boltzmann and by Ayrton and Perry are also given.

Gas.	\sqrt{D} . Boltzmann.	\sqrt{D} . Ayrton and Perry.	\sqrt{D} .	<i>n</i> .
Air	1·000295	1·00075	1·000293	1·0002927
Hydrogen	1·000132	1·00065	1·000132	1·0001387
Carbonic acid.....	1·000473	1·00115	1·000492	1·0004544
Carbonic oxide	1·000345	1·000347	1·0003350
Nitric protoxide	1·000497	1·000579	1·0005159
Olefiant gas	1·000656	1·000728	1·000720
Marsh-gas	1·000472	1·000476	1·000442
Vapour of carbon disulphide	}	1·00145	1·001478
Vapour of sulphurous acid.....		1·00260	1·00477	1·000704
Vapour of ether	1·00372	1·001537
Vapour of ethyl chloride...	1·00766	1·001174
Vapour of ethyl bromide...	1·00773	1·001218

K. Akad. der Wissenschaften in Wien, March 19, 1885.

FORMATION OF A STALACTITE BY VAPOUR. BY J. BROWN*.

The following curious phenomenon occurred during the electrolysis of the double chloride of aluminium and sodium fused in a small porcelain crucible provided with a porous partition. The anode was of carbon, and the cathode platinum-foil.

A considerable quantity of vapour was given off, especially from about the anode, forming a white smoke and depositing a white substance, doubtless mainly hydrated aluminium chloride, on the carbon rod, and about the mouth of the crucible, ultimately closing up the latter all but a small hole, through which the vapour poured rapidly. From this hole there grew out a beautifully delicate little tube about $1\frac{1}{2}$ inch long, and tapering from about $\frac{1}{8}$ inch at the base to $\frac{1}{10}$ inch in the middle of its length, after which it increased in diameter, and also flattened out owing to the vapour-jet coming close over the bend of the platinum-foil cathode, which seemed to cause, by some kind of eddy current, a flattening of the stream of vapour.

Soon afterwards the supply of vapour slackened, and there was a corresponding diminution in the size of the tube in the last quarter-inch of its length till the end became almost closed. The formation of this tube seems quite analogous to that of the ordinary tubular lime-carbonate stalactite deposited from dropping water by contact with the atmosphere; only we have here a tubular deposit of hydrated aluminium chloride by the combination, at the edge of the growing tube, of the water-vapour in the air with the anhydrous chloride contained in the vapour-stream.

Belfast, March 1885.

* Abstract of a paper read before the Belfast Natural History and Philosophical Society. Communicated by the Author.

MEASUREMENT OF STRONG ELECTRICAL CURRENTS.

BY JOHN TROWBRIDGE.

In 1871 I described in Silliman's Journal a new form of galvanometer which I called the Cosine Galvanometer. Six or seven years after the appearance of my paper, with a large woodcut of my instrument, the same instrument was re-invented in England by Mr. Obach, and the instrument now goes by the name of the Obach galvanometer in England, and is manufactured by the Messrs. Siemens, for the measurement of strong electrical currents.

In the prosecution of an investigation upon dynamo-electric machines I abandoned the cosine galvanometer in favour of a dynamometer which I invented, and a full description of which will be found in the Proceedings of the American Academy of Arts and Sciences, and also in the London Philosophical Magazine. It seemed to me then, and I have had no reason to change my belief, that a dynamometer is the most suitable instrument for measuring strong currents. I have lately, however, employed the cosine galvanometer for this purpose in the following manner:—The galvanometer is mounted so that its compass is at the centre of a large circle of wire, the plane of which is vertical and is in the magnetic meridian or in the plane of the needle of the compass, whatever that is. When the current from a dynamo machine is passed through the large vertical coil, which may consist of a single wire, the arrangement answers as a tangent galvanometer. I then connect the movable coil of the cosine galvanometer with a Daniell cell of known electromotive force, place in the same circuit a resistance so large that the battery resistance can be neglected, and having joined the poles in such a manner that the deflection produced by the coil of the cosine galvanometer shall be opposite to that produced by the current from the dynamo machine in the large outer coil, I incline the coil of the cosine galvanometer until the compass-needle is brought again to zero.

We then have, if we represent by F and F' the force produced at the centre of the coils by the current from the dynamo machine and by the Daniell cell, S and S' the respective currents, r and r' the radii, n and n' the number of coils in the two galvanometers, and H the horizontal force of magnetism:—

$$F = \frac{2\pi nSH}{r} = P = \frac{2\pi n'S'H}{r'} \cos \alpha, \text{ or } S = \frac{S'n' r}{n r'} \cos \alpha.$$

The strength of the current from the dynamo is thus simply obtained in terms of the current from the standard Daniell cell, and the method is independent of the strength of the earth's magnetism, or of the special field in which the instruments may be placed. The diameter of the outer coil can be diminished by employing Professor Brackett's method of passing the current through one coil in one direction and through an inside coil in another. In an experimental trial I employed simply a scaffolding of wood, upon which a single turn of wire was fixed as a vertical circle.—Silliman's *American Journal*, March 1885.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JUNE 1885.

XLV. *On the Electromagnetic Wave-surface.*

By OLIVER HEAVISIDE*.

MAXWELL showed (Electricity and Magnetism, vol. ii. art. 794) that his equations of electromagnetic disturbances, on the assumption that the electric capacity varies in different directions in a crystal, lead to the Fresnel form of wave-surface. There is no obscurity arising from the ignored wave of normal disturbance, because the very existence of a plane wave requires that there be none. In fact, the electric displacement and the magnetic induction are both in the wave-front, and are perpendicular to one another. The magnetic force and induction are parallel, on account of the constant permeability; whilst the electric force, though not parallel to the displacement, is yet perpendicular to the magnetic induction (and force); the normal to the wave-front, the electric force, and the displacement being in one plane. The ray is also in this plane, perpendicular to the electric force. There are of course two rays for (in general) every direction of wave-normal, each with separate electromagnetic variables to which the above remarks apply.

It is easily proved, and it may be legitimately inferred without a formal demonstration, from a consideration of the equations of induction, that if we consider the dielectric to be isotropic as regards capacity, but eolotropic as regards permeability, the same general results will follow, if we translate capacity to permeability, electric to magnetic force, and elec-

* Communicated by the Author.

tric displacement to magnetic induction. The three principal velocities will be $(c\mu_1)^{-\frac{1}{2}}$, $(c\mu_2)^{-\frac{1}{2}}$, and $(c\mu_3)^{-\frac{1}{2}}$, if c is the constant value of the capacity, and μ_1, μ_2, μ_3 are the three principal permeabilities. The wave-surface will be of the same character, only differing in the constants.

But a dielectric may be eolotropic both as regards capacity and permeability. The electric displacement is then a linear function of the electric force, and the magnetic induction another linear function of the magnetic force. The principal axes of capacity, or lines of parallelism of electric force and displacement, cannot, in the general case, be assumed to have any necessary relation to the principal axes of permeability, or lines of parallelism of magnetic force and induction. Disconnecting the matter altogether from the hypothesis that light consists of electromagnetic vibrations, we shall inquire into the conditions of propagation of plane electromagnetic waves in a dielectric which is eolotropic as regards both capacity and permeability, and determine the equation to the wave-surface.

For any direction of the normal (to the wave-front, understood) there are in general two normal velocities, *i. e.* there are two rays differently inclined to the normal whose ray-velocities and normal wave-velocities are different. And for any direction of ray there are in general two ray-velocities, *i. e.* two parallel rays having different velocities and wave-fronts.

In any wave (plane) the electric displacement and the magnetic induction must be always in the wave-front, *i. e.* perpendicular to the normal. But they are only exceptionally perpendicular to one another.

In any ray the electric force and the magnetic force are both perpendicular to the direction of the ray. But they are only exceptionally perpendicular to one another.

The magnetic force is always perpendicular to the electric displacement, and the electric force perpendicular to the magnetic induction. This of course applies to either wave. If we have to rotate the plane through the normal and the magnetic force through an angle θ to bring it to coincide with the magnetic induction, we must rotate the plane through the normal and the electric displacement through the same angle θ in the same direction to bring it to coincide with the electric force, the axis of rotation being the normal itself.

In the two waves having a common wave-normal, the displacement of either is parallel to the induction of the other. And in the two rays having a common direction, the magnetic force of either is parallel to the electric force of the other.

Nearly all our equations are symmetrical with respect to capacity and permeability; so that for every equation containing some electric variables there is a corresponding one, to be got by exchanging electric force and magnetic force, &c. And when the forces, inductions, &c. are eliminated, leaving only capacities and permeabilities, these may be exchanged in any formula without altering its meaning, although its immediate Cartesian expansion after the exchange may be entirely different, and only convertible to the former expression by long processes.

If either μ or c be constant, we have the Fresnel wave-surface. Perhaps the most important case besides these is that in which the principal axes of permeability are parallel to those of capacity. There are then six principal velocities instead of only three, for the velocity of a wave depends upon the capacity in the direction of displacement as well as upon the permeability in the direction of induction. For instance, if μ_1, μ_2, μ_3 and c_1, c_2, c_3 are the principal permeabilities and capacities, and the wave-normal be parallel to the common axis of μ_1 and c_1 , the other principal axes are the directions of induction and displacement, and the two normal velocities are $(c_2\mu_3)^{-\frac{1}{2}}$ and $(c_3\mu_2)^{-\frac{1}{2}}$.

The principal sections of the wave-surface in this case are all ellipses (instead of ellipses and circles, as in the one-sided Fresnel-wave); and two of these ellipses always cross, giving two axes of single-ray velocity. But should the ratio of the capacity to the permeability be the same for all the axes ($\mu_1/c_1 = \mu_2/c_2 = \mu_3/c_3$), the wave-surface reduces to a single ellipsoid, and any line is an optic axis. There is but one velocity, and no particular polarization. If the ratio is the same for two of the axes, the third is an optic axis.

Owing to the extraordinary complexity of the investigation when written out in Cartesian form (which I began doing, but gave up aghast), some abbreviated method of expression becomes desirable. I may also add, nearly indispensable, owing to the great difficulty in making out the meaning and mutual connections of very complex formulæ. In fact the transition from the velocity-equation to the wave-surface by proper elimination would, I think, baffle any ordinary algebraist, unassisted by some higher method, or at any rate by some kind of shorthand algebra. I therefore adopt, with some simplification, the method of vectors, which seems indeed the only proper method. But some of the principal results will be fully expanded in Cartesian form, which is easily done. And since all our equations will be either wholly scalar or wholly vector, the investigation is made independent of qua-

ternions by simply defining a scalar product to be so and so, and a vector product so and so. The investigation is thus a Cartesian one modified by certain simple abbreviated modes of expression.

I have long been of opinion that the sooner the much needed introduction of quaternion methods into practical mathematical investigations in Physics takes place the better. In fact every analyst to a certain extent adopts them: first, by writing only one of the three Cartesian scalar equations corresponding to the single vector equation, leaving the others to be inferred; and next, by writing the first only of the three products which occur in the scalar product of two vectors. This, systematized, is I think the proper and natural way in which quaternion methods should be gradually brought in. If to this we further add the use of the vector product of two vectors, immensely increased power is given, and we have just what is wanted in the three dimensional analytical investigations of electromagnetism, with its numerous vector magnitudes.

It is a matter of great practical importance that the notation should be such as to harmonize with Cartesian formulæ, so that we can pass from one to the other readily, as is often required in mixed investigations, without changing notation. This condition does not appear to me to be attained by Professor Tait's notation, with its numerous letter prefixes, and especially by the $-S$ before every scalar product, the negative sign being the cause of the greatest inconvenience in transitions. I further think that Quaternions, as applied to Physics, should be established more by definition than at present; that scalar and vector products should be defined to mean such or such operations, thus avoiding some extremely obscure and quasi-metaphysical reasoning, which is quite unnecessary.

The first three sections of the following preliminary contain all we want as regards definitions; most of the rest of the preliminary consists of developments and reference-formulæ, which, were they given later, in the electromagnetic problem, would inconveniently interrupt the argument, and much lengthen the work.

Scalars and Vectors.—In a scalar equation every term is a scalar, or algebraic quantity, a mere magnitude; and $+$ and $-$ have the ordinary signification. But in a vector equation every term stands for a vector, or directed magnitude, and $+$ and $-$ are to be understood as compounding like velocities, forces, &c. Putting all vectors upon one side, we have the general form

$$A + B + C + D + \dots = 0;$$

where A, B, \dots , are any vectors, which, if n in number, may be represented, since their sum is zero, by the n sides of a polygon. Let A_1, A_2, A_3 be the three ordinary scalar components of A referred to any set of three rectangular axes, and similarly for the other vectors. This notation saves multiplication of letters. Then the above equation stands for the three scalar equations

$$\left. \begin{aligned} A_1 + B_1 + C_1 + D_1 + \dots &= 0, \\ A_2 + B_2 + C_2 + D_2 + \dots &= 0, \\ A_3 + B_3 + C_3 + D_3 + \dots &= 0. \end{aligned} \right\}$$

The $-$ sign before a vector simply reverses its direction—that is, negatives its three components.

According to the above, if i, j, k be rectangular vectors of unit length, we have

$$A = iA_1 + jA_2 + kA_3 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

&c.; if A_1, A_2, A_3 be the components of A referred to the axes of i, j, k . That is, A is the sum of the three vectors iA_1, jA_2, kA_3 , of lengths A_1, A_2, A_3 parallel to i, j, k respectively.

Scalar Product.—We define AB thus,

$$AB = A_1B_1 + A_2B_2 + A_3B_3, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and call it the scalar product of the vectors A and B . Its magnitude is that of $A \times$ that of $B \times$ the cosine of the angle between them. Thus, by (1) and (2),

$$A_1 = Ai, \quad A_2 = Aj, \quad A_3 = Ak;$$

and in general, N being any unit vector, AN is the scalar component of A parallel to N , or, briefly, the N component of A . Similarly,

$$i^2 = 1, \quad j^2 = 1, \quad k^2 = 1,$$

because i and i are parallel and of length unity, &c. And

$$ij = 0, \quad jk = 0, \quad ki = 0,$$

because i and j , for instance, are perpendicular. Notice that $AB = BA$.

We have also

$$A = \frac{A^2}{A} = \frac{A^3}{A^2} = \&c.,$$

and

$$\frac{1}{A} \text{ or } A^{-1} = \frac{A}{A^2} = \frac{A^2}{A^3} = \&c.$$

Thus A^{-1} has the same direction as A ; its length is the reciprocal of that of A .

Vector Product.—We define VAB thus,

$$VAB = i(A_2B_3 - A_3B_2) + j(A_3B_1 - A_1B_3) + k(A_1B_2 - A_2B_1), \quad (3)$$

and call VAB the vector product of A and B. Its magnitude is that of $A \times$ that of $B \times$ the sine of the angle between them. Its direction is perpendicular to A and to B with the usual conventional relation between positive directions of translation and of rotation (the vine system). Thus, $Vij = k$; $Vjk = i$; $Vki = j$. Notice that $VAB = -VBA$, the direction being reversed by reversing the order of the letters; or by exchanging A and B in (3) we negative each term.

Hamilton's ∇ . The operator

$$\nabla = i \frac{d}{dx} + j \frac{d}{dy} + k \frac{d}{dz} \quad . \quad . \quad . \quad . \quad (4)$$

may, since the differentiations are scalar, be treated as a vector, of course with either a scalar or a vector to follow it. If it operate on a scalar P we have the vector

$$\nabla P = i \frac{dP}{dx} + j \frac{dP}{dy} + k \frac{dP}{dz}, \quad . \quad . \quad . \quad . \quad (5)$$

whose three components are dP/dx , &c. If it operate on a vector A, we have, by (2), the scalar product

$$\nabla A = \frac{dA_1}{dx} + \frac{dA_2}{dy} + \frac{dA_3}{dz}, \quad . \quad . \quad . \quad . \quad (6)$$

and, by (3), the vector product

$$V\nabla A = i \left(\frac{dA_3}{dy} - \frac{dA_2}{dz} \right) + j \left(\frac{dA_1}{dz} - \frac{dA_3}{dx} \right) + k \left(\frac{dA_2}{dx} - \frac{dA_1}{dy} \right). \quad (7)$$

The scalar product ∇A is the divergence of the vector A, the amount leaving the unit volume, if it be a flux. The vector product (7) is the curl of A, which will occur below. There are three remarkable theorems relating to ∇ , viz.

$$P_2 - P_1 = \int_1^2 \nabla P ds, \quad . \quad . \quad . \quad . \quad (8)$$

$$\int A ds = \iint B dS, \quad . \quad . \quad . \quad . \quad (9)$$

$$\iint C dS = \iiint \nabla C dv. \quad . \quad . \quad . \quad . \quad (10)$$

Starting with P, a single-valued scalar function of position, the rise in its value from any point to another is expressed in (8) as the line-integral, along any line joining the points, of $\nabla P ds$, the scalar product of ∇P , and ds the vector element of the curve.

Then passing from an unclosed to a closed curve, let A be any vector function of position (single-valued of course). Its

line-integral round the closed curve is expressed in (9) as the surface-integral over any surface bounded by the curve of another vector B , which $= \nabla A$. BdS is the scalar product of B and the vector element of surface dS , whose direction is defined by its unit normal.

Finally, passing from an unclosed to a closed surface, (10) expresses the surface-integral of any vector C over the closed surface (normal positive outward), as the volume-integral of its divergence within the included space.

Linear Vector Operators.—If H be the magnetic force at a point, B the induction, E the electric force, and D the displacement, all vectors, then

$$B = \mu H, \text{ and } D = cE / 4\pi \quad . \quad . \quad . \quad (11)$$

express the relation of B to H and of D to E in a dielectric medium. If it be isotropic as regards displacement, c is the electric capacity; and if it be isotropic as regards induction, μ is the magnetic permeability; c and μ are then constants, if the medium be homogeneous, or scalar functions of position, if it be heterogeneous.

We shall not alter the form of the above equations in the case of eolotropy, when c and μ become linear operators. For instance, the induction will always be μH , to be understood as a definite vector, got from H another vector, in a manner fully defined by (in case we want the developments) the following equations (not otherwise needed). Let H_1, \dots , and B_1, \dots , be the components of H and B referred to any rectangular axes. Then

$$\left. \begin{aligned} B_1 &= \mu_{11}H_1 + \mu_{12}H_2 + \mu_{13}H_3, \\ B_2 &= \mu_{21}H_1 + \mu_{22}H_2 + \mu_{23}H_3, \\ B_3 &= \mu_{31}H_1 + \mu_{32}H_2 + \mu_{33}H_3, \end{aligned} \right\} \quad . \quad . \quad . \quad (12)$$

where μ_{11} &c. are constants, which may have any values not making HB negative; with the identities $\mu_{12} = \mu_{21}$, &c.

Or,

$$B_1 = \mu_1 H_1, \quad B_2 = \mu_2 H_2, \quad B_3 = \mu_3 H_3; \quad . \quad . \quad . \quad (13)$$

when the components are those referred to the principal axes of permeability, μ_1, μ_2, μ_3 being the principal permeabilities, all positive.

Inverse Operators.—Since $B = \mu H$, we have $H = \mu^{-1}B$, where μ^{-1} is the operator inverse to μ . When referred to the principal axes, we have

$$\mu'_1 = \frac{1}{\mu_1}, \quad \mu'_2 = \frac{1}{\mu_2}, \quad \mu'_3 = \frac{1}{\mu_3} \quad . \quad . \quad . \quad (14)$$

But when referred to any rectangular axes, we have

$$\mu'_{11} = \frac{\mu_{11}\mu_{22} - \mu_{12}^2}{\mu_1\mu_2\mu_3}, \quad \mu'_{12} = \frac{\mu_{13}\mu_{23} - \mu_{12}\mu_{33}}{\mu_1\mu_2\mu_3}, \text{ \&c.} \quad (15)$$

by solution of (12). The accents belong to the inverse coefficients. The rest may be written down symmetrically, by cyclical changes of the figures. In the index surface the operators are inverse to those in the wave-surface.

Conjugate Property.—The following property will occur frequently. A and B being any vectors

$$A\mu B = B\mu A, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

or the scalar product of A and μB equals that of B and μA . It only requires writing out the full scalar products to see its truth, which results from the identities $\mu_{12} = \mu_{21}$, &c. Similarly,

$$A\mu c B = \mu A c B = c\mu A B, \text{ \&c.},$$

$$AB = A\mu\mu^{-1}B = \mu A\mu^{-1}B, \text{ \&c.},$$

where in the first line c is another self-conjugate operator.

D is expressed in terms of E similarly to (12) by coefficients c_{11} , c_{12} , &c.; or, as in (13), by the principal capacities c_1 , c_2 , c_3 .

Theorem.—The following important theorem will be required. A and B being any vectors,

$$\mu_1\mu_2\mu_3 VAB = \mu V\mu A\mu B. \quad . \quad . \quad . \quad . \quad (17)$$

For completeness a proof is now inserted, adapted from that given by Tait. Since VAB is perpendicular to A and B, by definition of a vector product, therefore

$$AVAB = 0, \text{ and } BVAB = 0,$$

by definition of a scalar product. Therefore

$$A\mu\mu^{-1}VAB = 0, \text{ and } B\mu\mu^{-1}VAB = 0,$$

by introducing $\mu\mu^{-1} = 1$. Hence

$$\mu A\mu^{-1}VAB = 0, \text{ and } \mu B\mu^{-1}VAB = 0$$

by the conjugate property; that is, $\mu^{-1}VAB$ is perpendicular to μA and to μB . Or

$$h\mu^{-1}VAB = V\mu A\mu B,$$

where h is a scalar. Or

$$hVAB = \mu V\mu A\mu B$$

by operating by μ . To find h , multiply by any third vector C (not to be in the same plane as A and B), giving

$$hCVAB = C\mu V\mu A\mu B;$$

therefore

$$h = \frac{\mu C V \mu A \mu B}{C V A B}$$

by the conjugate property. Now expand this quotient of two scalar products, and it will be found to be independent of what vectors A, B, C may be. Choose them then to be i, j, k , three unit vectors parallel to the principal axes of μ . Then

$$h = \frac{\mu_3 k V \mu_1 i \mu_2 j}{k V i j} = \mu_1 \mu_2 \mu_3,$$

by the ijk properties before mentioned. This proves (17).

Transformation-Formula.—The following is very useful. A, B, C being any vectors,

$$V A V B C = B(CA) - C(AB). \quad . \quad . \quad . \quad (18)$$

Here CA and AB are scalar products, merely set in brackets to separate distinctly from the vectors B and C they multiply. This formula is evident on expansion.

The Equations of Induction.—E and H being the electric and magnetic forces at a point in a dielectric, the two equations of induction are

$$\text{curl } H = c \dot{E}, \quad . \quad . \quad . \quad . \quad . \quad (19)$$

$$-\text{curl } E = \mu \dot{H}; \quad . \quad . \quad . \quad . \quad . \quad (20)$$

c and μ being the capacity and permeability operators, and curl standing for $V \nabla$ as defined in equation (7). Let Γ and G be the electric and the magnetic current, then

$$\Gamma = c \dot{E} / 4\pi, \quad G = \mu \dot{H} / 4\pi. \quad . \quad . \quad . \quad . \quad (21)$$

The dot, as usual, signifies differentiation to the time. The electric energy is $E c E / 8\pi$ per unit volume, and the magnetic energy $H \mu H / 8\pi$ per unit volume. If A is Maxwell's vector potential of the electric current, we have also

$$\text{curl } A = \mu H, \quad E = -\dot{A}. \quad . \quad . \quad . \quad . \quad (21 a)$$

Similarly, we may make a vector Z the vector potential of the magnetic current, such that

$$-\text{curl } Z = c E, \quad H = -\dot{Z}. \quad . \quad . \quad . \quad . \quad (22)$$

The complete magnetic energy, by a well-known transformation, of any current system may be expressed in the two ways,

$$T = \Sigma H \mu H / 8\pi = \frac{1}{2} \Sigma A \Gamma,$$

the Σ indicating summation through all space. Similarly, the electric energy, if there be no electrification, may be

written in the two ways,

$$U = \Sigma E c E / 8\pi = \frac{1}{2} \Sigma Z G.$$

If there be electrification, we have also another term to add, the real *electrostatic* energy, in terms of the scalar potential and electrification. And if there be impressed electric force in the dielectric, part of G will be imaginary magnetic current, analogous to the imaginary electric current which may replace a system of intrinsic magnetization.

Plane Wave.—Let there be a plane wave in the medium. Its direction is defined by its normal. Let then N be the vector normal of unit length, and z be distance measured along the normal. If v be the velocity of the wave-front, the rate the disturbance travels along the normal, or the component parallel to the normal of the actual velocity of propagation of the disturbance, we have

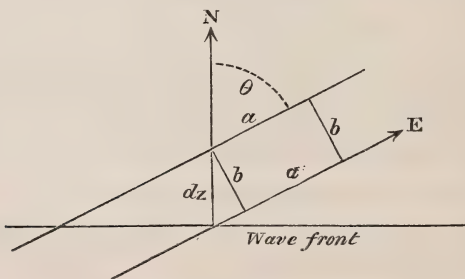
$$H = f(z - vt)$$

if the wave be a positive one, as we shall suppose, giving

$$-v \frac{d}{dz} = \frac{d}{dt}, \quad \dots \dots \dots (23)$$

applied to H or E .

Next, examine what the operator $V\nabla$ or curl becomes when, as at present, the disturbance is assumed not to change direction, but only magnitude, as we pass along the normal. Apply the theorem of Version (9) to the elementary rectangular area bounded by two sides parallel to E of length a , and



two sides of length b perpendicular to E and in the same plane as E and the normal N . Since its area is ab , and $b = dz \sin \theta$, and the two sides b contribute nothing to the line-integral, we find that

$$\text{curl} = VN \frac{d}{dz}, \quad \dots \dots \dots (24)$$

applied to E or H or other vectors, in the case of a plane

wave. Using this, and (23), in the equations of induction (19), (20), they become

$$\mathbf{V}\mathbf{N}\frac{d\mathbf{H}}{dz} = -vc\frac{d\mathbf{E}}{dz},$$

$$\mathbf{V}\mathbf{N}\frac{d\mathbf{E}}{dz} = v\mu\frac{d\mathbf{H}}{dz}.$$

Here, since the z differentiation is scalar, and occurs on both sides, it may be dropped, giving us

$$\mathbf{V}\mathbf{N}\mathbf{H} = -vc\mathbf{E}, \quad . \quad . \quad . \quad . \quad . \quad (25)$$

$$\mathbf{V}\mathbf{N}\mathbf{E} = v\mu\mathbf{H}. \quad . \quad . \quad . \quad . \quad . \quad (26)$$

The induction and the displacement are therefore necessarily in the wave-front, by the definition of a vector product, being perpendicular to \mathbf{N} . Also the displacement is perpendicular to the magnetic force, and the induction is perpendicular to the electric force.

Index Surface.—Let $\sigma = \frac{\mathbf{N}}{v}, \quad . \quad . \quad . \quad . \quad . \quad (27)$

be a vector parallel to the normal, whose length is the reciprocal of the normal velocity v . It is the vector of the index surface. By (25) and (26) we have

$$c\mathbf{E} = -\mathbf{V}\sigma\mathbf{H}, \text{ therefore } -\mathbf{E} = c^{-1}\mathbf{V}\sigma\mathbf{H}; \quad . \quad . \quad (28)$$

and

$$\mu\mathbf{H} = \mathbf{V}\sigma\mathbf{E}, \text{ therefore } \mathbf{H} = \mu^{-1}\mathbf{V}\sigma\mathbf{E}. \quad . \quad . \quad (29)$$

Now use the theorem (17). Then, if

$$m = \mu_1\mu_2\mu_3, \quad n = c_1c_2c_3 \quad . \quad . \quad . \quad . \quad (30)$$

be the products of the principal permeabilities and capacities, the theorem gives, applied to (28) and (29),

$$-n\mathbf{E} = \mathbf{V}c\sigma c\mathbf{H}, \quad . \quad . \quad . \quad . \quad . \quad (31)$$

$$m\mathbf{H} = \mathbf{V}\mu\sigma\mu\mathbf{E}. \quad . \quad . \quad . \quad . \quad . \quad (32)$$

Putting the value of \mathbf{H} given by (32) in (28) first, and then the value of \mathbf{E} given by (31) in (29), we have

$$-m\mathbf{E} = c^{-1}\mathbf{V}\sigma\mathbf{V}\mu\sigma\mu\mathbf{E}, \quad . \quad . \quad . \quad . \quad (33)$$

$$-n\mathbf{H} = \mu^{-1}\mathbf{V}\sigma\mathbf{V}c\sigma c\mathbf{H}. \quad . \quad . \quad . \quad . \quad (34)$$

To these apply the transformation-formula (18), giving

$$-mc\mathbf{E} = \mu\sigma(\sigma\mu\mathbf{E}) - \mu\mathbf{E}(\sigma\mu\sigma) \quad . \quad . \quad . \quad (33a)$$

and

$$-n\mu\mathbf{H} = c\sigma(\sigma c\mathbf{H}) - c\mathbf{H}(\sigma c\sigma), \quad . \quad . \quad . \quad (34a)$$

where the bracketed quantities are scalar products. Put in this form

$$\{(\sigma\mu\sigma)\mu - mc\}E = \mu\sigma(\sigma\mu E), \quad . \quad . \quad . \quad (35)$$

$$\{(\sigma c\sigma)c - n\mu\}H = c\sigma(\sigma c H), \quad . \quad . \quad . \quad (36)$$

and perform on them the inverse operations to those contained in the $\{\}$'s, dividing also by the scalar products on the right sides. Then

$$\frac{E}{\sigma\mu E} = \frac{\mu\sigma}{(\sigma\mu\sigma)\mu - mc}, \quad . \quad . \quad . \quad (37)$$

$$\frac{H}{\sigma c H} = \frac{c\sigma}{(\sigma c\sigma)c - n\mu}. \quad . \quad . \quad . \quad (38)$$

Operate by c on (37) and by μ on (38), and transfer all operators to the denominators on the right. Then

$$\frac{cE}{\sigma\mu E} = \frac{\sigma}{(\sigma\mu\sigma)c^{-1} - m\mu^{-1}} = \beta_1 \text{ say}, \quad . \quad . \quad (39)$$

$$\frac{\mu H}{\sigma c H} = \frac{\sigma}{(\sigma c\sigma)\mu^{-1} - nc^{-1}} = \beta_2 \text{ say}. \quad . \quad . \quad (40)$$

[It should be noted that, in thus transferring operators, care should be taken to do it properly, otherwise it had better not be done at all. Thus, we have by (37),

$$\beta_1 = c \frac{\mu\sigma}{(\sigma\mu\sigma)\mu - mc}, \text{ or } \beta_1 = c\{(\sigma\mu\sigma)\mu - mc\}^{-1}\mu\sigma,$$

and the left c and the right μ are to go inside the $\{\}$. Operate by c^{-1} and then again by $\{\}^{+1}$, thus cancelling the $\{\}^{-1}$, giving

$$\mu\sigma = \{(\sigma\mu\sigma)\mu - mc\}c^{-1}\beta_1.$$

Here we can move c^{-1} inside, giving

$$\mu\sigma = \{(\sigma\mu\sigma)\mu c^{-1} - m\}\beta_1;$$

and now operating by μ^{-1} , it may be moved inside, giving

$$\sigma = \{(\sigma\mu\sigma)c^{-1} - m\mu^{-1}\}\beta_1,$$

as in (39).]

We can now, by (39) and (40), get as many forms of the index equation as we please. We know that the displacement is perpendicular to the normal, and so is the induction. Hence

$$\sigma\beta_1 = 0, \quad \sigma\beta_2 = 0; \quad . \quad . \quad . \quad (41)$$

where β_1 and β_2 are the above vectors, [(39) and (40)], are two equivalent equations of the index surface.

Also, operate on (39) by $\sigma\mu c^{-1}$, and on (40) by $\sigma c\mu^{-1}$, and

the left members become unity, by the conjugate property ; hence

$$\mu\sigma c^{-1}\beta_1=1, \quad c\sigma\mu^{-1}\beta_2=1 \quad . \quad . \quad . \quad (42)$$

are two other forms of the index equation. (41) and (42) are the simplest forms. More complex forms are created with that surprising ease which is characteristic of these operators; but we do not want any more. When expanded, the different forms look very different, and no one would think they represented the same surface. This is also true of the corresponding Fresnel surface, which is comparatively simple in expression. In any equation we may exchange the operators μ and c .

Put $\sigma=Nv^{-1}$ in any form of index equation, and we have the velocity equation, a quadratic in v^2 giving the two velocities of the wave-front. And if we put $Nv=p$, making thus p a vector parallel to the normal of length equal to the velocity, it will be the vector of the surface which is the locus of the foot of the perpendicular from the origin upon the tangent-plane to the wave-surface.

By (33 a), remembering that σ is parallel to the normal, we see that

$$\left. \begin{array}{l} \text{or,} \quad cE, \mu E, \text{ and } \mu N \text{ are in one plane;} \\ \quad \quad E, N, \text{ and } \mu^{-1}cE \text{ are in one plane.} \end{array} \right\} \quad . \quad . \quad (43)$$

And by (34 a),

$$\left. \begin{array}{l} \text{or,} \quad \mu H, cN, \text{ and } cH \text{ are in one plane;} \\ \quad \quad H, N, \text{ and } c^{-1}\mu H \text{ are in one plane.} \end{array} \right\} \quad . \quad . \quad (44)$$

These conditions expanded, give us the directions of the electric force and displacement, the magnetic force and induction, for a given normal. We may write the second of (43) thus,

$$NV \frac{D}{c} \frac{D}{\mu} = 0; \quad . \quad . \quad . \quad . \quad (45)$$

and the second of (44) thus,

$$NV \frac{B}{c} \frac{B}{\mu} = 0; \quad . \quad . \quad . \quad . \quad (46)$$

and as these differ only in the substitution of B for D , we see that the induction of either ray is parallel to the displacement of the other ; that is, the two directions of induction in the wave-front are the two directions of displacement.

The Wave-Surface.—Since the velocity-surface with the vector $p=vN$ is the locus of the foot of the perpendicular on the tangent-plane to the wave-surface, we have, if ρ be the

vector of the wave-surface,

$$p\rho = p^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (47)$$

But σ the vector of the index-surface being $= Nv^{-1} = pv^{-2}$, we have by (47), dividing it by v^2 ,

$$\sigma\rho = 1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (48)$$

To find the wave-surface, we must therefore let σ be variable and eliminate it between (48) and any one of the index equations. This is not so easy as it may appear.

General considerations may lead us to the conclusion that the equation to the wave-surface and that to the index-surface may be turned one into the other by the simple process of inverting the operators, turning c into c^{-1} and μ into μ^{-1} . Although this will be verified later, any form of index equation giving a corresponding form of wave by inversion of operators, yet it must be admitted that this requires proof. That it is true when one of the operators c or μ is a constant does not prove that it is also true when we have the inverse compound operator $\{(\sigma c \sigma)\mu^{-1} - nc^{-1}\}^{-1}$ containing both c and μ , neither being constant. I have not found an easy proof. This will not be wondered at when the similar investigations of the Fresnel surface are referred to. Professor Tait, in his 'Quaternions,' gives two methods of finding the wave-surface; one from the velocity equation, the other from the index equation. The latter is rather the easier, but cannot be said to be very obvious, nor does either of them admit of much simplification. The difficulty is of course considerably multiplied when we have the two operators to reckon with. I believe the following transition from index to wave cannot be made more direct, or shorter, except of course by omission of steps, which is not a real shortening.

$$\text{Given} \quad \frac{cE}{\sigma\mu E} = \beta_1 = \frac{\sigma}{(\sigma\mu\sigma)c^{-1} - m\mu^{-1}}, \quad (49) = (39) \text{ bis,}$$

$$\sigma\beta_1 = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (50) = (41) \text{ bis,}$$

$$\rho\sigma = 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad (51) = (48) \text{ bis.}$$

Eliminate σ and get an equation in ρ . We have also

$$\mu\sigma c^{-1}\beta_1 = 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad (52) = (42) \text{ bis}$$

which will assist later.

By (49) we have

$$\sigma = (\sigma\mu\sigma)c^{-1}\beta_1 - m\mu^{-1}\beta_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad (53)$$

Multiply by β_1 and use (50); then

$$0 = (\sigma\mu\sigma)(\beta_1 c^{-1}\beta_1) - m(\beta_1 \mu^{-1}\beta_1). \quad . \quad . \quad . \quad . \quad . \quad . \quad (54)$$

By differentiation, σ being variable, and therefore β_1 also,

$$0 = 2(d\sigma\mu\sigma)(\beta_1 c^{-1}\beta_1) + 2(\sigma\mu\sigma)(d\beta_1 c^{-1}\beta_1) - 2m(d\beta_1\mu^{-1}\beta_1). \quad (55)$$

Also, differentiating (53),

$$d\sigma = 2(d\sigma\mu\sigma)c^{-1}\beta_1 + (\sigma\mu\sigma)dc^{-1}\beta_1 - md\mu^{-1}\beta_1;$$

and, multiplying this by $2\beta_1$, gives

$$2\beta_1 d\sigma = 4(d\sigma\mu\sigma)\beta_1 c^{-1}\beta_1 + 2(\sigma\mu\sigma)(d\beta_1 c^{-1}\beta_1) - 2m(d\beta_1\mu^{-1}\beta_1). \quad (56)$$

Subtract (55) from (56) and halve the result; thus obtaining

$$\beta_1 d\sigma = (d\sigma\mu\sigma)(\beta_1 c^{-1}\beta_1),$$

or

$$\{\beta_1 - (\beta_1 c^{-1}\beta_1)\mu\sigma\} d\sigma = 0. \quad . \quad . \quad . \quad (57)$$

In the last five equations it will be understood that $d\sigma$ and $d\beta_1$ are differential vectors, and that $d\sigma\mu\sigma$ is the scalar product of $d\sigma$ and $\mu\sigma$, &c.; also in getting (56) from the preceding equation we have $\beta_1 dc^{-1}\beta_1 = \beta_1 c^{-1}d\beta_1 = d\beta_1 c^{-1}\beta_1$, &c. Equation (57) is the expression of the result of differentiating (50),

$$d(\sigma\beta_1) = d\sigma\beta_1 + \sigma d\beta_1 = 0,$$

with $d\beta_1$ eliminated.

Now (57) shows that the vector in the $\{\}$ is perpendicular to $d\sigma$ the variation of σ . But by (51) we also have, on differentiation,

$$\rho d\sigma = 0. \quad . \quad . \quad . \quad . \quad . \quad (58)$$

Hence ρ and the $\{\}$ vector in (57) must be parallel. This gives

$$h\rho = \beta_1 - (\beta_1 c^{-1}\beta_1)\mu\sigma, \quad . \quad . \quad . \quad . \quad . \quad (59)$$

where h is a scalar. If we multiply this by $c^{-1}\beta_1$ and use (52), we obtain

$$\rho c^{-1}\beta_1 = 0; \quad . \quad . \quad . \quad . \quad . \quad (60)$$

or, by (49), giving β_1 in terms of cE ,

$$\rho E = 0, \quad . \quad . \quad . \quad . \quad . \quad (61)$$

a very important landmark. The ray is perpendicular to the electric force.

Similarly, if we had started from, (instead of (49), (50), and (52)), the corresponding H equations, viz.,

$$\frac{\mu H}{\sigma c H} = \beta_2 = \frac{\sigma}{(\sigma c \sigma)\mu^{-1} - nc^{-1}},$$

$$\sigma\beta_2 = 0, \quad c\sigma\mu^{-1}\beta_2 = 1,$$

with of course the same equation (51) connecting ρ and σ , we

should have arrived at

$$h'\rho = \beta_2 - (\beta_2\mu^{-1}\beta_2)c\sigma; \quad . \quad . \quad . \quad (62)$$

h' being a constant, corresponding to (59); of this no separate proof is needed, as it amounts to exchanging μ and c and turning E into H , to make (39) become (40). And from (62), multiplying it by $\mu^{-1}\beta_2$, we arrive at

$$\rho\mu^{-1}\beta_2 = 0, \text{ or } \rho H = 0, \quad . \quad . \quad . \quad (63)$$

corresponding to (61). The ray is thus perpendicular both to the electric and to the magnetic force. The first half of the demonstration is now completed, but before giving the second half we may notice some other properties.

Thus, to determine the values of the scalar constants h and h' . Multiply (59) by σ , and use (50) and (51); then

$$h = -(\beta_1 c^{-1} \beta_1)(\sigma \mu \sigma) = -m(\beta_1 \mu^{-1} \beta_1),$$

the second form following from (54). Insert in (59), then

$$\rho = \frac{\mu\sigma}{\sigma\mu\sigma} - \frac{\beta_1}{m(\beta_1\mu^{-1}\beta_1)} \quad . \quad . \quad . \quad (64)$$

gives ρ explicitly in terms of $\mu\sigma$ and β_1 , the latter of which is known in terms of the former by (49). Multiply this by $\mu^{-1}\beta_1$, using (50); then

$$\rho\mu^{-1}\beta_1 = -\frac{1}{m}. \quad . \quad . \quad . \quad (65)$$

Similarly we shall find

$$h' = -n(\beta_2 c^{-1} \beta_2), \quad . \quad . \quad . \quad (66)$$

giving

$$\rho = \frac{c\sigma}{\sigma c \sigma} - \frac{\beta_2}{n(\beta_2 c^{-1} \beta_2)}; \quad . \quad . \quad . \quad (67)$$

and, corresponding to (65), we shall have

$$\rho c^{-1} \beta_2 = -\frac{1}{n}. \quad . \quad . \quad . \quad (68)$$

Now to resume the argument, stopped at equation (63). Up to equation (59) the work is plain and straightforward, according to rule in fact, being merely the elimination of the differentials, and the getting of an equation between ρ and σ . What to do next is not at all obvious. From (59), or from (64), the same with h eliminated, we may obtain all sorts of scalar products containing ρ and β_1 , and if we could put β_1 explicitly in terms of ρ , (60) or (65) would be forms of the wave-surface equation. From the purely mathematical point of view no direct way presents itself; but (61) and (63), considered physically as well as mathematically, guide us at once to the second half of the transformation from the index

to the wave equation. As, at the commencement, we found the induction and the displacement to be perpendicular to the normal, so now we find that the corresponding forces are perpendicular to the ray. There was no difficulty in reaching the index equation before, when we had a single normal with two values of v the normal velocity, and two rays differently inclined to the normal. There should then be no difficulty, by parallel reasoning, in arriving at the wave-surface equation from analogous equations which express that the ray is perpendicular to the magnetic and electric forces, considering two parallel rays travelling with different ray-velocities with two differently inclined wave-fronts.

Now, as we got the index equation from

$$VNH = -vcE, \quad . \quad . \quad . \quad . \quad . \quad (25) \text{ bis}$$

$$VNE = v\mu H, \quad . \quad . \quad . \quad . \quad . \quad (26) \text{ bis}$$

we must have two corresponding equations for one ray-direction. Let M be a unit vector defining the direction of the ray, and w be the ray-velocity, so that

$$\rho = wM. \quad . \quad . \quad . \quad . \quad . \quad (69)$$

Operate on (25) and (26) by VM , giving

$$VMVN H = -vVMcE,$$

$$VMVNE = vVM\mu H.$$

Now use the formula of transformation (18), giving

$$N(HM) - H(MN) = -vVMcE,$$

$$N(EM) - E(MN) = vVM\mu H.$$

But $HM=0$ and $EM=0$, as proved before. Also $v=w(MN)$, or the wave-velocity is the normal component of the ray-velocity. Hence

$$H = wVMcE, \quad . \quad . \quad . \quad . \quad . \quad (70)$$

$$-E = wVM\mu H, \quad . \quad . \quad . \quad . \quad . \quad (71)$$

which are the required analogues of (25) and (26). Or, by (69),

$$H = V\rho cE, \quad . \quad . \quad . \quad . \quad . \quad (72)$$

$$-E = V\rho\mu H \quad . \quad . \quad . \quad . \quad . \quad (73)$$

are the analogues of (28) and (29). The rest of the work is plain. Eliminating E and H successively, we obtain

$$0 = E + V\rho\mu V\rho cE,$$

$$0 = H + V\rho c V\rho\mu H;$$

and, using the theorem (17), these give

$$0 = E + mV\rho V\mu^{-1}\rho\mu^{-1}cE,$$

$$0 = H + nV\rho Vc^{-1}\rho c^{-1}\mu H;$$

which, using the transformation-formula (18), become

$$0 = E + m\mu^{-1}\rho(\mu^{-1}\rho cE) - \mu^{-1}cE(\rho\mu^{-1}\rho)m,$$

$$0 = H + nc^{-1}\rho(c^{-1}\rho\mu H) - c^{-1}\mu H(\rho c^{-1}\rho)n;$$

or, rearranging, after operating by μ and c respectively,

$$\{(\rho\mu^{-1}\rho)mc - \mu\}E = m\rho(\mu^{-1}\rho cE),$$

$$\{(\rho c^{-1}\rho)n\mu - c\}H = n\rho(c^{-1}\rho\mu H).$$

Or

$$\frac{E}{\mu^{-1}\rho cE} = \frac{\rho}{(\rho\mu^{-1}\rho)c - m^{-1}\mu} = \gamma_1, \text{ say,} \quad . \quad . \quad (74)$$

$$\frac{H}{c^{-1}\rho\mu H} = \frac{\rho}{(\rho c^{-1}\rho)\mu - n^{-1}c} = \gamma_2, \text{ say.} \quad . \quad . \quad (75)$$

These give us the four simplest forms of equation to the wave. For, since $\rho E = 0 = \rho H$, we have

$$\rho\gamma_1 = 0, \quad \rho\gamma_2 = 0. \quad . \quad . \quad . \quad (76)$$

Also, operating on (74) by $\mu^{-1}\rho c$ and on (75) by $c^{-1}\rho\mu$ we get

$$\mu^{-1}\rho c\gamma_1 = 1, \quad c^{-1}\rho\mu\gamma_2 = 1, \quad . \quad . \quad (77)$$

two other forms.

γ_1 and γ_2 differ from β_1 and β_2 merely in the change from σ to ρ , and in the inversion of the operators. The two forms of wave (76) are analogous to (41), and the two forms (77) analogous to (42), inverting operators and putting ρ for σ .

Similarly, if the wave-surface equation be given and we require that of the index-surface, we must impose the same condition $\rho\sigma = 1$ as before, and eliminate ρ . This will lead us to

$$\sigma c\gamma_1 = 0, \quad \sigma\mu\gamma_1 = -m, \quad . \quad . \quad . \quad (78)$$

corresponding to (60) and (65); and

$$\sigma\mu\gamma_2 = 0, \quad \sigma c\gamma_2 = -n, \quad . \quad . \quad . \quad (79)$$

corresponding to (63) and (68); and the firsts of (78) and (79) are equivalent to

$$\sigma cE = 0, \quad \sigma\mu H = 0;$$

or the displacement and the induction are perpendicular to the normal. This completes the first half of the process; the second part would be the repetition of the already given investigation of the index equation.

The vector rate of transfer of energy being $VEH/4\pi$ in general, when a ray is solitary, its direction is that of the transfer of energy. It seems reasonable, then, to define the direction of a ray, whether the wave is plane or not, as perpendicular to the electric and the magnetic forces. On this understanding, we do not need the preliminary investigation of the index-surface, but may proceed at once to the wave-surface by the investigation (69) to (77), following equations (25) and (26).

The following additional useful relations are easily deducible :—From (25) and (26) we get

$$\sigma = \frac{VcE\mu H}{EcE}; \quad . \quad . \quad . \quad . \quad . \quad (80)$$

and from (72) and (73),

$$\rho = \frac{VEH}{EcE} \quad . \quad . \quad . \quad . \quad . \quad (81)$$

Also, from either set,

$$EcE = H\mu H, \quad . \quad . \quad . \quad . \quad . \quad (82)$$

expressing the equality of the electric to the magnetic energy per unit volume (strictly, at a point).

Some Cartesian Expansions.—In the important case of parallelism of the principal axes of capacity and permeability, the full expressions for the index or the wave-surface equations may be written down at once from the scalar product abbreviated expressions. Thus, taking any equation to the wave, as the first of (76), for example, $\rho\gamma_1=0$, γ being given in (74), take the axes of coordinates parallel to the common principal axes of c and μ ; so that we can employ c_1, c_2, c_3 , the principal capacities, and μ_1, μ_2, μ_3 the principal permeabilities in the three components of γ_1 . We then have, x, y, z being the coordinates of ρ ,

$$\frac{x^2}{(\rho\mu^{-1}\rho)c_1 - m\mu_1} + \frac{y^2}{(\rho\mu^{-1}\rho)c_2 - m\mu_2} + \frac{z^2}{(\rho\mu^{-1}\rho)c_3 - m\mu_3} = 0, \quad (83)$$

where

$$\rho\mu^{-1}\rho = \frac{x^2}{\mu_1} + \frac{y^2}{\mu_2} + \frac{z^2}{\mu_3}.$$

In (83) we may exchange the c 's and μ 's, getting the second of (76). Similarly the first of (77) gives

$$\frac{\mu_1^{-1}c_1x^2}{(\rho\mu^{-1}\rho)c_1 - m\mu_1} + \frac{\mu_2^{-1}c_2y^2}{(\rho\mu^{-1}\rho)c_2 - m\mu_2} + \frac{\mu_3^{-1}c_3z^2}{(\rho\mu^{-1}\rho)c_3 - m\mu_3} = 1 \quad (84)$$

as another form, in which, again, the μ 's and c 's may be exchanged (not forgetting to change m into n) to give a fourth form.

These reduce to the Fresnel surface if either $\mu_1 = \mu_2 = \mu_3$ or $c_1 = c_2 = c_3$.

Let $x=0$ to find the sections in the plane yz . The first denominator in (83) gives

$$\left(\frac{y^2}{\mu_2} + \frac{z^2}{\mu_3}\right)c_1 - \frac{1}{\mu_2\mu_3} = 0, \text{ or } y^2c_1\mu_3 + z^2c_1\mu_2 = 1,$$

representing an ellipse, semiaxes

$$v_{13} = (c_1\mu_3)^{-\frac{1}{2}} \text{ and } v_{12} = (c_1\mu_2)^{-\frac{1}{2}}.$$

The other terms give

$$\left(\frac{y^2}{\mu_2} + \frac{z^2}{\mu_2}\right)(c_3y^2 + c_2z^2) = \frac{y^2}{\mu_1\mu_2} + \frac{z^2}{\mu_1\mu_3}.$$

Or

$$y^2\mu_1c_3 + z^2\mu_1c_2 = 1.$$

An ellipse, semiaxes $v_{31} = (c_3\mu_1)^{-\frac{1}{2}}$ and $v_{21} = (c_2\mu_1)^{-\frac{1}{2}}$. Similarly, in the plane zx the sections are ellipses whose semiaxes are v_{21} , v_{23} , and v_{12} , v_{32} , where for brevity $v_{rs} = (c_r\mu_s)^{-\frac{1}{2}}$; and in the plane xy , the ellipses have semiaxes v_{31} , v_{32} , and v_{13} , v_{12} .

In one of the principal planes two of the ellipses intersect, giving four places where the two members of the double surface unite.

If $c_1/\mu_1 = c_2/\mu_2 = c_3/\mu_3$, we have a single ellipsoidal wave-surface whose equation is

$$\frac{x^2}{v_{23}^2} + \frac{y^2}{v_{31}^2} + \frac{z^2}{v_{12}^2} = 1. \quad . \quad . \quad . \quad (85)$$

Now, of course, $v_{12} = v_{21}$, &c.

When the μ and c axes are not parallel, we cannot immediately write down the full expansion of the wave-surface equation. Proceed thus:—Taking $\rho\gamma_1 = 0$ as the equation, let

$$R = m(\rho\mu^{-1}\rho), \text{ and } \alpha = m^{-1}\gamma_1;$$

then, by (74) and (76),

$$\rho \frac{\rho}{Rc - \mu} = 0, \text{ or } \rho\alpha = 0,$$

where

$$\rho = (Rc - \mu)\alpha. \quad . \quad . \quad . \quad . \quad . \quad . \quad (86)$$

R is a scalar. If α_1 , α_2 , α_3 are the three components of α referred to any rectangular axes, and x , y , z the components of ρ , we have, by (86) and (12),

$$\begin{aligned} x &= (Rc_{11} - \mu_{11})\alpha_1 + (Rc_{12} - \mu_{12})\alpha_2 + (Rc_{13} - \mu_{13})\alpha_3, \\ y &= (Rc_{21} - \mu_{21})\alpha_1 + (Rc_{22} - \mu_{22})\alpha_2 + (Rc_{23} - \mu_{23})\alpha_3, \\ z &= (Rc_{31} - \mu_{31})\alpha_1 + (Rc_{32} - \mu_{32})\alpha_2 + (Rc_{33} - \mu_{33})\alpha_3; \end{aligned}$$

from which $\alpha_1, \alpha_2, \alpha_3$ may be solved in terms of x, y, z ; thus

$$\alpha_1 = a_{11}x + a_{12}y + a_{13}z,$$

$$\alpha_2 = a_{21}x + a_{22}y + a_{23}z,$$

$$\alpha_3 = a_{31}x + a_{32}y + a_{33}z;$$

where, by using (15),

$$a_{11} = \frac{(Rc_{22} - \mu_{22})(Rc_{33} - \mu_{33}) - (Rc_{23} - \mu_{23})^2}{\Delta},$$

$$a_{12} = \frac{(Rc_{13} - \mu_{13})(Rc_{23} - \mu_{23}) - (Rc_{12} - \mu_{12})(Rc_{33} - \mu_{33})}{\Delta};$$

and the rest by symmetry. Then, since

$$\rho\alpha = x\alpha_1 + y\alpha_2 + z\alpha_3 = 0,$$

we get the full expansion. Δ need not be written fully, as it goes out. The equation may be written symmetrically, thus,

$$0 = 1 + mn(\rho\mu^{-1}\rho)(\rho c^{-1}\rho) - \{x^2(c_{22}\mu_{33} + c_{33}\mu_{22} - 2c_{23}\mu_{23}) + \dots \\ + 2xy(c_{13}\mu_{23} + c_{23}\mu_{13} - c_{12}\mu_{33} - c_{33}\mu_{12}) + \dots\}, \quad (87)$$

where the coefficients of y^2, z^2, yz , and zx are omitted. Here $m = \mu_1\mu_2\mu_3$ and $n = c_1c_2c_3$; whilst

$$\rho c^{-1}\rho = c'_{11}x^2 + c'_{22}y^2 + c'_{33}z^2 + 2c'_{12}xy + 2c'_{23}yz + 2c'_{31}zx,$$

where c'_{11}, \dots , are the inverse coefficients. See equation (15). The expansion of $\rho\mu^{-1}\rho$ is exactly similar, using the inverse μ coefficients.

If in (87) we for every c or μ write the reciprocal coefficients, we obtain the equation to the index-surface; that is, supposing x, y, z then to be the components of σ instead of ρ . And, since $\sigma v = N$, the unit wave normal, we have the velocity equation as follows, in the general case,

$$0 = \frac{N\mu N}{m} \cdot \frac{NcN}{n} + v^4 - v^2\{N_1^2(c'_{22}\mu'_{33} + c'_{33}\mu'_{22} - 2c'_{23}\mu'_{23}) + \dots \\ + 2N_1N_2(c'_{13}\mu'_{23} + c'_{23}\mu'_{13} - c'_{12}\mu'_{33} - c'_{33}\mu'_{12}) + \dots\}. \quad (88)$$

in which N_1, N_2, N_3 are the components of N , or the direction-cosines of the normal. To show the dependence of v^2 upon the capacity and permeability perpendicular to N , take $N_1 = 1, N_2 = 0, N_3 = 0$, which does not destroy generality, because in (88) the axes of reference are arbitrary. Then (88) reduces to

$$v^4 - (c'_{22}\mu'_{33} + c'_{33}\mu'_{22} - 2c'_{23}\mu'_{23})v^2 \\ + (c'_{22}c'_{33} - c'^2_{23})(\mu'_{22}\mu'_{33} - \mu'^2_{23}) = 0.$$

When the μ and c axes are parallel, and their principal axes are those of reference, we have

$$0 = \frac{N\mu N}{m} \cdot \frac{NcN}{n} + v^4 - v^2 \{ N_1^2(v_{23}^2 + v_{32}^2) + N_2^2(v_{31}^2 + v_{13}^2) + N_3^2(v_{12}^2 + v_{21}^2) \}, \quad \dots \quad (89)$$

where

$$N\mu N = \mu_1 N_1^2 + \mu_2 N_2^2 + \mu_3 N_3^2,$$

with a similar expression for NcN , and $v_{23} = (c_2 \mu_3)^{-\frac{1}{2}}$, &c., as before. The solution is

$$v^2 = \frac{1}{2} N_1^2(v_{23}^2 + v_{32}^2) + \frac{1}{2} N_2^2(v_{31}^2 + v_{13}^2) + \frac{1}{2} N_3^2(v_{12}^2 + v_{21}^2) \pm \frac{1}{2} \sqrt{X}, \quad (90)$$

where

$$X = N_1^4 u_1^2 + N_2^4 u_2^2 + N_3^4 u_3^2 - 2(N_1^2 N_2^2 u_1 u_2 + N_2^2 N_3^2 u_2 u_3 + N_3^2 N_1^2 u_1 u_3),$$

in which

$$u_1 = v_{23}^2 - v_{32}^2, \quad u_2 = v_{31}^2 - v_{13}^2, \quad u_3 = v_{12}^2 - v_{21}^2. \quad \dots \quad (91)$$

Take $u_1 = 0$, or $c_2 / \mu_2 = c_3 / \mu_3$; the two velocities are then

$$N_1^2 v_{23}^2 + N_2^2 v_{31}^2 + N_3^2 v_{12}^2, \quad \text{and} \quad N_1^2 v_{23}^2 + N_2^2 v_{13}^2 + N_3^2 v_{21}^2,$$

reducing to one velocity v_{23} when $N_1 = 1$.

If, further, $u_2 = 0$, or $u_3 = 0$, making $c_1 / \mu_1 = c_2 / \mu_2 = c_3 / \mu_3$, $X = 0$ always, and

$$v^2 = N_1^2 v_{23}^2 + N_2^2 v_{31}^2 + N_3^2 v_{12}^2, \quad \dots \quad (92)$$

is the single value of the square of velocity of wave-front.

Directions of E, H, D, and B.—We may expand (45) to obtain an equation for the two directions of the induction and displacement. Thus, since

$$\frac{D}{c} = i(c'_{11} D_1 + c'_{12} D_2 + c'_{13} D_3) + j(c'_{21} D_1 + c'_{22} D_2 + c'_{23} D_3) + k(c'_{31} D_1 + c'_{32} D_2 + c'_{33} D_3),$$

$$\frac{D}{\mu} = i(\mu'_{11} D_1 + \mu'_{12} D_2 + \mu'_{13} D_3) + j(\mu'_{21} D_1 + \mu'_{22} D_2 + \mu'_{23} D_3) + k(\mu'_{31} D_1 + \mu'_{32} D_2 + \mu'_{33} D_3),$$

$$N = iN_1 + jN_2 + kN_3,$$

the determinant of the coefficients of i, j, k equated to zero gives the required equation. When the principal axes of μ and c are parallel, the equation greatly simplifies, being then

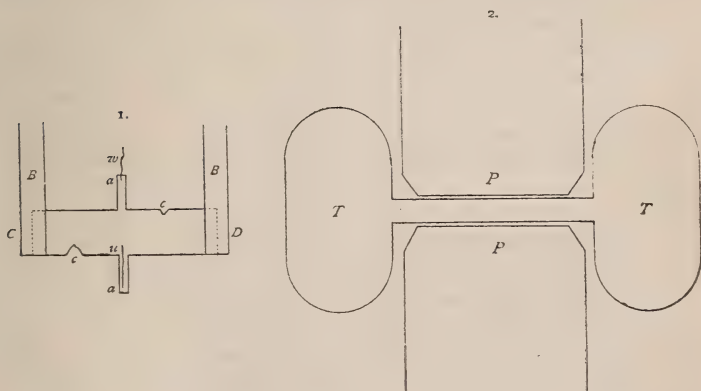
$$0 = \frac{N_1 u_1}{D_1} + \frac{N_2 u_2}{D_2} + \frac{N_3 u_3}{D_3}, \quad \dots \quad (93)$$

where u_1, \dots , are the same differences of squares of principal

velocities, as in (91). For D_1 &c. write B_1 &c.; and we have the same equations for the induction directions. For D_1 , &c., write $c_1 E_1$, &c., and the resulting equation gives the directions of E . For D_1 , &c., write $\mu_1 H_1$, &c., and the resulting equation gives the directions of H .

XLVI. *On the Rotation of the Equipotential Lines of an Electric Current by Magnetic Action.* By E. H. HALL, Instructor in Physics at Harvard College*.

IN this article the results will be given of experiments made during the month of August 1883, and at intervals since, in the Physical Laboratory of Harvard College. The substances which have been chiefly examined are copper, zinc, certain of their alloys, and iron and steel. Some mention will be made also of gold, cobalt, nickel, bismuth, and antimony. In most cases when possible the metal was used in the form of a thin strip, about 1.1 centim. wide and about 3 centim. long, between the two pieces of brass B, B (fig. 1), which, soldered to the ends of the strip, served as electrodes for the entrance and escape of the main current. To the arms a, a , about 2 millim. wide and perhaps 7 millim. long, were soldered



the wires w, w , which led to a Thomson galvanometer. The notches c, c , show how adjustment was secured. The strip thus prepared was fastened to a plate of glass by means of a cement of beeswax and rosin, all the parts shown in the figure being imbedded in and covered by this cement, which was so

* -From Silliman's American Journal, February 1885.

hard and stiff as to be quite brittle at the ordinary temperature of the air.

The plate of glass bearing the strip of metal so imbedded was, when about to be tested, placed with B, B, vertical in the narrow part of a tank whose horizontal section is shown in fig. 2. This tank, T T, containing the plate of glass with the metal strip was placed between the poles P, P, of the electromagnet. The tank was filled with water, which was sometimes at rest and sometimes flowing. By this means the temperature of the strip of metal was under tolerable control, and the inconvenience from thermoelectric effects at *a* and *a* considerably lessened. The diameter of the plane circular ends of the pole-pieces P, P, is about 3.7 centim.

The general method of most of the experiments to be mentioned did not differ much from that described in the *Philosophical Magazine* for November 1880. The intensity of the magnetic field was estimated as before, by the impulse given to a galvanometer-needle when a small coil in connection with the galvanometer was suddenly removed from the field. This impulse was compared with that given to the same needle by the current obtained by turning an earth-inductor of known dimensions.

The direct current through the strip under examination was measured by means of a tangent galvanometer. The transverse current was measured by means of a Thomson galvanometer, the reduction-factor of this instrument usually being determined by passing through it a current of known strength a few minutes before and a few minutes after each set of observations on the transverse current.

The rotational powers will then be given in ostensibly absolute measure; but an uncertainty of several per cent. attaches to the values given owing to uncertainty in regard to the following quantities:—1st, the thickness of the strips examined; 2nd, the dimensions of the small test-coil used for getting strength of magnetic field; 3rd, the horizontal intensity of the earth's magnetism; 4th, the reduction factors of the tangent-galvanometer used; 5th, the magnitude of the direct effect exerted by the electromagnet upon the Thomson galvanometer at a distance of about 50 feet.

One of the most troublesome operations in these experiments is that of determining the thickness of the metal strips examined. Owing to the inevitable slight roughness of the surface, direct measurements with calipers is likely to give too great a thickness. On the other hand, the density of many specimens is subject to considerable uncertainty, and therefore the indirect method by means of weight and density,

which is the method used, cannot be applied with full confidence. Moreover the latter method gives, at best, only the average thickness of the strip, whereas I have heretofore assumed, and in this article still assume, that the effective thickness of the strip is the thickness of that part which lies between the two arms a, a . My practice of late has been, therefore, to determine the average thickness by the weight and density method, making use of the best data available, and to estimate the thickness between the arms by adding to, or subtracting from, the average thickness, according as the calipers indicated the thickness at that place to be greater or less than the average. In the case of several of the strips to be mentioned hereafter this correction was somewhat carelessly made, and there may be an inaccuracy of four or five per cent. in the estimated "effective thickness." With other strips much care was taken in this respect, and it is believed that the uncertainty in regard to the density of the metal is the greatest source of error in determining the thickness in these cases. Particulars will be given as the strips are in turn described.

The 5th source of error was very troublesome in the experiments upon certain alloys. The galvanometer was unfortunately so placed that not only the magnitude, but even the direction of this effect of the electromagnet might be varied when, by any means, the galvanometer-needle was turned a few degrees from its ordinary position of rest.

Copper, Zinc, and their Alloys.

More than three years ago (B. A. Report, 1881) I found that if the rotational power of copper is called $-$, that of zinc is $+$. At the same time a specimen of brass, exact composition unknown, had been found to lie between copper and zinc in this respect, but nearer the copper, having in fact a small $-$ rotating power.

Through the kindness of Prof. Trowbridge and Mr. E. K. Stevens, I had at command in the summer of 1883 several alloys of copper and zinc in widely varying proportions. Specimens of these alloys had been analyzed chemically by Mr. Stevens; but as they had lain for some time in an exposed position after he had finished his work upon them, I feared the labels upon them might not be intact, and Mr. G. D. Moore, of the Harvard class of 1884, has been kind enough to make new analyses for me, determining both the copper and the zinc in specimens furnished him in the form of thin strips such as were used in my own experiments. Mr. Moore found:—

Specimen.	Copper. per cent.	Zinc. per cent.
A . . .	99.9	
B . . .	81.08	18.51
C . . .	72.86	27.02
D . . .	66.85	33.04
F . . .	5.87	93.79
G . . .	a trace	99.54

Specimen E, which contained apparently about 50 per cent. copper, was so brittle that I did not succeed in getting it rolled into a thin sheet.

Most, if not all, of these specimens were annealed one or more times during the process of rolling. None of the strips examined, however, were annealed after the final rolling. All of the strips which were used were cut in such a way that the arms *a, a* extended in that direction in which the strips passed through the rolls.

As it is a somewhat troublesome matter to determine accurately the density of a thin strip of metal, and as my immediate purpose did not demand great accuracy in this respect, it seemed allowable to estimate the density of the alloys from their composition. After certain rough experiments the density 8.9 was assumed for the copper, and 7.2 for the zinc. Assuming, what we know to be not strictly true, that the density of an alloy decreases regularly as its amount of zinc per unit mass increases, we find :—

Alloy.	Density.
B . . .	8.6
C . . .	8.4
D . . .	8.3
F . . .	7.3

The description of particular strips will now be given.

A. (No. 1.)

Length of main strip when weighed . . .	4.20 cm.
Width of main strip when weighed . . .	1.07 cm.
Area, including that of the arms . . .	4.98 cm. sq.
Weight202 grm.
Density	8.9
Average thickness from above00456 cm.

By calipers the thickness appeared to be at one end .0046; other end .0044; between arms .0048; average .0046. Take, then, for true thickness between arms, .00476.

The measurement with calipers in this case was not made with great care, and the correction here applied is open to some suspicion.

With this strip the space between the brass strips B, B (fig. 1) was about 3·2 millim.

B. (No. 1.)

Length of main strip when weighed .	4·58 cm.
Width of main strip when weighed .	1·08 cm.
Area, including that of the arms . .	5·32 cm. sq.
Weight	·184 gm.
Density	8·58
Average thickness from these data . .	·00403 cm.

From the indications of the calipers I concluded that the thickness between the arms was about $1\frac{1}{2}$ per cent. greater than the average thickness.

Hence thickness between the arms ·00409 cm. This value, like that given for A, may be wrong to the extent of several per cent. Distance between strips B, B about 3·2 cm.

C. (No. 1.)

Length of main strip when weighed .	3·75 cm.
Width of main strip when weighed .	1·08 cm.
Area of whole strip when weighed . .	4·39 cm. sq.
Weight	·1085 gm.
Density	8·44
Average thickness from these data . .	·00292 cm.

By calipers, thickness at one end ·0034 ; at other end ·0032 ; between arms ·0034 ; average ·00333 cm. Take, then, for true thickness between arms, ·00299 cm. Distance between strips B, B, about 3·2 cm.

D. (No. 1.)

Length of main strip when weighed .	3·86 cm.
Width of main strip when weighed .	1·07 cm.
Area of whole strip	4·45 cm. sq.
Weight	·1081 gm.
Density	8·33
Average thickness from these data . .	·00291 cm.

By calipers, thickness at one end ·0030 ; at other end ·0030 ; between arms ·0032 ; average ·00307 cm. Take for true thickness between arms, ·00304 cm. Distance between strips B, B, about 3·2 cm.

F. (No. 1.)

Length of main strip when weighed .	3.70 cm.
Width of main strip when weighed .	1.06 cm.
Area of whole strip when weighed .	4.28 cm. sq.
Weight2915 grm.
Density	7.3
Average thickness from the data . .	.00934 cm.

By calipers, thickness at one end .0098 ; at other end .0100 ; between arms .0100; average .00993 cm. Take for true thickness between arms, .00941 cm. Distance between strips B, B, about 3.2 cm.

G. (No. 1.)

Length of main strip when weighed .	4.17 cm.
Width of main strip when weighed .	1.09 cm.
Area of whole strip when weighed .	5.03 cm. sq.
Weight1206 grm.
Density	7.2
Average thickness from these data . .	.00333 cm.

From a somewhat careful use of the calipers it appeared that the thickness between the arms was about $4\frac{1}{2}$ per cent greater than the average thickness.

Take, then, for thickness between arms, .00348 cm. Distance between strips B, B, about 3.2 cm.

The main object in the experiments upon these metals and their alloys was to determine whether the alloys would range themselves according to any simple law, so that the magnitude of the rotational power in any alloy might be inferred from its known proportions of copper and zinc. Some attempt was made, moreover, to determine the effect of change of temperature in different specimens.

It will be seen that the intensity of the magnetic field was kept nearly the same throughout the experiments of August upon copper, zinc, and their alloys. The strength of the direct current was, moreover, of about the same magnitude in all cases, except the experiments of August 11th on copper. On that day the current used was less than one half as strong as that used later. The same strip of copper was tested again August 29th with a current of the usual strength ; and the agreement between the results obtained under so widely different conditions is quite close.

In the following table and throughout this article the C. G. S. system is used wherever no statement is made to the contrary. The following symbols used with the tables need explanation :—

C means current along line C D (fig. 1).

M means intensity of magnetic field perpendicular to plane of paper in fig. 1.

R. P. means *rotatory power*, which is numerically defined by the expression $D \frac{E}{C \times M}$, in which D is the thickness of the metal strip, and E is the electromotive force resulting from C and M which maintains the *transverse current**. The R. P. is considered *positive* when E is of such a character as to send a *positive* transverse current in that direction in which the conducting strip tends to move under the action of the main current and the magnetic force.

The temperatures referred to in the tables are those indicated by a thermometer placed in the water-tank containing the plates when under examination (fig. 2).

Copper, Zinc, and their Alloys.

Strip.	Composition acc. to chem. anal.		Date.	Temp.	M.	C.	R. P. $\times 10^{15}$	R. P. near 24°C $\times 10^{15}$.	Composition, &c.†	
	Per cent. Coppr.	Per cent. Zinc.							Per cent. Coppr.	Per cent. Zinc.
A {	100 (99.9)		1883. Aug. 11	$^{\circ} \text{C}$ 3	5700	.0619	(-521)			
	"		"	4.8	5640	.0819	(-530)			
	"		"	21.5	5560	.0876	(-520)			
	"		Aug. 29	24.5	6030	.1906	(-519)	(-520)		
B {	81.3 (81.08)	18.7 (18.51)	Aug. 18	25	5750	.1800	(-404)	(-404)	91.3	8.7
C {	73 (72.86)	27 (27.02)	Aug. 17	5.1	5680	.1810	(-246)			
	"	"	"	22.8	5700	.1830	(-250)	(-250)	80	20
D {	67 (66.85)	33 (33.04)	Aug. 13	6	5830	.1680	(-178)			
	"	"	"	25	5930	.1680	(-166)	(-166)	73.6	26.4
F {	6 (5.87)	94 (93.79)	Aug. 17	4	5640	.1790	(+527)			
	"	"	"	24?	5690	.1780	(+496)	(+496)	24.3	75.7
G {	0.0 (trace)	100 (99.54)	Aug. 15	4.4	6150	.1860	(+838)			
			"	25	6130	.1850	(+830)			
			"	22.4	6210	.1870	(+809)	(+820)		

* The R. P. of this article is the *reciprocal* of the quantity $\frac{M \times V}{E'}$ used in my article in this Journal for November 1880.

† Composition of alloys which *a priori* might have been expected to have these values of R. P.

This table shows the results of experiments with the metal strips thus far described in detail. The last two columns give approximately the composition of plates which would have the observed rotatory powers if copper and zinc in uniting retained their individual powers unaltered. It will be perceived that in every case the proportion of copper demanded upon this basis is greater than the proportion in the actual plate.

At the time these experiments were made I supposed plate B to contain about 92 per cent. of copper, which would have furnished an exception to this rule. Thinking some serious error might have been made in estimating the thickness of this plate, or that of the copper plate, or the zinc, I prepared with very great care plates No. 2 of A, B, and G. These new plates were similar to those already described, but were in length between B and B about 2.2 cm. As the vacation was now over, and my opportunities for experimental work were limited, I contented myself with making, in the main, comparative experiments with these plates. I did not re-determine the horizontal intensity of the earth's magnetism, and I made no attempt to determine the direct effect of the electromagnet upon the Thomson galvanometer, with which the transverse current was observed. I knew that this direct effect was small; and I endeavoured to arrange matters in such a way that it would affect all the new results with A, B, and G in the same direction and to nearly the same extent. The intensity of the magnetic field in the experiments with these plates was about the same as in the previous experiments with the alloys. This intensity was determined, as usual, on October 20th, when the effect of change of temperature upon the plate B No. 2 was tested, but on October 13th, when A No. 2 and B No. 2 were compared, and on October 27th, when A No. 2 and G No. 2 were compared, this determination was not considered necessary. On October 13th A No. 2 was tested first, then B No. 2, then A No. 2 again. This arrangement tended to make the result of the comparison independent of any progressive diminution in the strength of the current operating the electromagnet, and it saved much time. On October 27th the same method was followed.

I shall not attempt to give the results of these experiments in absolute measure, but shall write the results obtained on an arbitrary scale.

Specimen.	Per cent. of Copper.	Per cent. of Zinc.	Temp.	R. P.
A. No. 2.	99.9	100(-)
B. „	81.08	18.51	21°	76.1(-)
„ „	„	„	4°	78.7(-)
G. „	...	99.54	...	158.8(+)

A No. 2 and B No. 2 were compared at about 25° C.,
A No. 2 and G No. 2 at about 19° C.

The values of R. P. given in the first table are in the following proportion :—

	R. P.
A. No. 1 . .	100 (-)
B. „ . .	77.6 (-)
C. „ . .	157.5 (+)

This agreement was regarded as highly satisfactory, all things considered ; and the apparent exception which, as stated above, had been noted in the case of B, appeared to be confirmed. The result of the chemical analysis, however, showed that this alloy, which, according to the label found upon it, contained nearly 92 per cent. of copper, really had the composition given in the tables above. Alloy B, then, does conform to the rule that the alloys of copper and zinc have rotating powers nearer to that of copper than the composition of the alloys would have led one to expect.

Influence of Temperature.

It will be seen that in the case of copper, zinc, and all the alloys except C, a fall of temperature appears to cause a slight increase in the numerical value of the rotating power. It is possible that upon further trial the apparent exception furnished by C would disappear. It does not, on the other hand, seem probable that the agreement in a particular direction of five cases out of six is entirely accidental. In the case of D the apparent change caused by fall of temperature is very considerable ; but the observations made with this alloy were particularly unsatisfactory, the needle of the Thomson galvanometer being quite unsteady, while the total effect to be measured was small.

The experiments upon iron and steel were in the main repetitions of those already published, but made with more care.

Soft Iron.

The dimensions of the strip of soft iron used in the experiments were roughly as follows :—

Length between terminals B, B (fig. 1) . . . 2.9 cm.

Width 1.06 cm.

Thickness0041 cm.

Like the strip used in 1882* it was obtained through the kindness of Prof. Langley, of the Allegheny Observatory, and the two strips had probably nearly the same composition and character.

Although the test made in 1882 was not satisfactory, it seemed to me to indicate that the R. P. of soft iron was slightly greater at high than at low magnetic intensities, and I hazarded the opinion that future experiments would prove this to be the case. This opinion has scarcely been justified by the result. The table given below does indeed appear to indicate a very slight increase in the R. P. as M rises from 801 to 3264, and again as M rises from 3264 to 5835, but the increase in the numbers, as observed, is too slight to deserve any confidence.

Date.	Temp.	M.	C.	R. P.	Estimated R.P. at 28°.
Aug. 22, '83.	6.2	791.4	.1771	$+7147 \times 10^{-15}$	
"	28.2	797.6	.1742	+8302	
"	28.3	804.6	.1794	+8304	
	28.25	801.1		8303	$+8290 \times 10^{-15}$
Aug. 23.	28.1	3259	.1728	8306	
"	28.4	3274	.1702	8289	
"	27.65	3260	.1696	8306	
	28.05	3264		8300	$+8297 \times 10^{-15}$
Aug. 24.	28.7	5797	.1839	8391	
"	28.1	5833	.1842	8310	
"	28.0	5875	.1853	8245	
	28.27	5835		8315	$+8300 \times 10^{-15}$
Aug. 27.	27.25	8626	.1894	8139	
"	28.35	8690	.1898	8261	
"	27.95	8636	.1911	8200	
	27.85	8651		8200	$+8208 \times 10^{-15}$

Fall of R. P. for 1° fall of temperature equals approximately $\frac{2}{3}$ per cent.

* Silliman's American Journal, March 1883; and Philosophical Magazine for May 1883.

On the other hand, the decrease in the observed value of the R. P. as M rises from 5835 to 8651, though slight, seems to me strong evidence of an actual falling off in the value of the R. P. of iron at magnetic intensities.

In any case it appears, and this was one of the important questions to be answered by the experiments, that the R. P. of iron under magnetic forces of varying intensity is more nearly constant than the R. P. of nickel, for according to previous experiments* the R. P. of nickel in a magnetic field of intensity 8700 is many per cent., perhaps 20, less than in a magnetic field of intensity 2000.

The one trial made at low temperature confirmed the inference drawn from a similar trial made the year before†, that a fall of 1° C diminishes the R. P. in soft iron about $\frac{2}{3}$ per cent. It is with these experiments as a basis that I have ventured to make the slight changes necessary to deduce from the observed results the values of the R. P. at 28°.

A test was made with this strip of soft iron for a permanent change in the direction of the equipotential lines. A similar experiment with tempered steel the year before had been successful, and the description of the method I will quote from a previous article (*Philosophical Magazine* for May 1883, p. 345). "This plate, with the usual electrical connections, and with a current flowing through it, was placed in the usual position between the poles of the electromagnet, the magnet current was turned on, then off, and the plate removed from between the poles in order to avoid the action of the very considerable residual magnetism of the electromagnet. A reading of the Thomson galvanometer in the transverse circuit was now made, then the plate was replaced between the poles and the current turned on again, but in the opposite direction. The magnet current being again interrupted, the plate was again removed from the field and another reading of the Thomson galvanometer was made." Making this experiment with the soft iron and with a battery of 50 cells, the result was a negative one. If any permanent effect was produced, it was probably a small part of one per cent. of the temporary effect produced while the plate was subjected to the magnet's action.

Steel.

The steel used in this series of experiments was obtained from Montgomery and Co., New York, and was designated by the letters "F. C. R.," which stand for "French Cold

* *Phil. Mag.* Sept. 1881.

† *Phil. Mag.* May 1883.

Rolled." This steel, as it came from the dealers, was pliable and soft enough to be quite readily cut with a pocket-knife. The first strip to be described was tested in this "natural" condition.

No. 1 (*untempered*).

Length of main strip when weighed	. 4.13 cm.
Width of main strip when weighed	. 1.10 cm.
Area of whole strip when weighed	. 4.88 sq. cm.
Weight	4.21 grm.
Density	7.9
Average thickness from these data	. .01092 cm.
Estimated thickness between arms	. .01116 cm.
Distance from B to B, about	. 3.1 cm.

No. 2 (*tempered*).

The dimensions of this strip were not so accurately taken. It was so brittle that I felt obliged to use caution in applying the calipers to it. It appeared to be of about the same thickness as No. 7, and I assumed it to have exactly the same thickness. The distance from B to B was about 2.6 cm.

Results obtained with these two strips are shown in the following table:—

	Date.	Temp.	M.	C.	R. P.
No. 1.	Nov. 24.	18°·7	1647	·0970	+12000 × 10 ⁻¹⁵
"	"	2·5	1629	·1007	+11230 × "
		<hr/> 16·2			<hr/> 770
No. 2.	Dec. 1.	18°·0	1602	·0870	+32720 × "
"	"	1·9	1587	·0908	+30800 × "
		<hr/> 16·1			<hr/> 1920

Decrease for 1° fall of temperature, approximately ·4 per cent.

The magnitude of the rotational coefficient in the soft steel is about $1\frac{1}{2}$ times, and that in tempered steel about 4 times as great as that in the soft iron. This agrees very well with the conclusion drawn from the hasty experiments of the year before, when a piece of steel from a clock-spring was used. When a sensitive galvanometer is used, an ordinary permanent horse-shoe magnet of half-inch bar produces an easily discernible effect in the strip of tempered steel.

It should be stated that two or three months before the

trials which furnished this table were made, both these steel strips had been subjected, in the usual position, to a magnetizing force of about 8500 intensity. It does not seem probable that the results just recorded were seriously affected by this previous experience of the steel.

This first magnetization was for the purpose of detecting, if possible, a *permanent* effect, such as had been observed a year before in a piece of tempered clock-spring, but had been vainly looked for in soft iron.

No. 2 showed a permanent effect equal to about $1\frac{1}{2}$ per cent. of the temporary effect, a result agreeing very well with that obtained with the tempered clock-spring. No. 1 showed a permanent effect relatively, and even absolutely, larger than that in No. 2. It was about 5 per cent. of the temporary effect. In both cases the tests made were hasty, and the results obtained are only approximately correct. I believe it has been observed that residual magnetization is, under certain conditions, greater in soft than in hard steel.

Gold.

The only experiment of importance made with gold was a test for permanent effect. The strip used was the one experimented with the year before, a description of which has already been published (*Philosophical Magazine* for May 1883, p. 342). The permanent effect, if any was produced, must have been a very small part of one per cent. of the temporary effect.

Cobalt.

In the '*Philosophical Magazine*' for September 1831 I said:—"No thin strips of the metal [cobalt] being at hand, a slice was sawn from a small block of moderately pure cast cobalt and worked into the form of a cross. To the extremity of each arm of this cross was soldered a thin strip of copper . . . for the purpose of making the electrical connections. The cross of cobalt with the copper strips attached was now fastened with hard cement [beeswax and rosin] to a strip of glass," &c. It was the same piece of cobalt, reduced now with file and emery-paper to a thickness of about $\cdot 0062$ cm., that served during this new series of experiments. The central portion of the cross was about 2 millim. square, and the arms, about 3 millim. long and 2 millim. wide, projected from it at right angles.

The main object of the recent test was to determine the effect produced in cobalt by change of temperature. It is assumed that the intensity of the magnetic field remained con-

stant during these experiments. It is possible, however, that it diminished one or two per cent. during the series. The error introduced in this way is of slight consequence for the present purpose. Again, the direct effect of the electro-magnet in disturbing the needle of the Thomson galvanometer was neglected, together with certain other particulars which may have introduced a constant error of several per cent. into the values obtained for the rotatory power.

No. of Exp.	Date.	Temp.	M.	C.	R. P.
1.	Dec. 22, 1883.	3°	3463	·1251	+2092 × 10 ⁻¹⁵
3.	"	1·3	"	·1243	+2061
		<hr/> 2	"		<hr/> 2076
2.	"	18	"	·1238	+2441
4.	"	18	"	·1244	+2390
		<hr/> 18			<hr/> 2415

Decrease of R. P. for fall of 1° C. is approximately $\frac{2}{15}$ per cent.

Bismuth.

I can only confirm what Prof. Righi* has already published concerning the rotational effect in this metal. The rotational power appeared to be about 260 times as great numerically as that of tempered steel, and of the opposite sign.

The slice of bismuth used was obtained, shaped, and mounted in much the same manner as the slice of cobalt already described. The length and breadth of the bismuth cross were somewhat greater than the corresponding dimensions of the cobalt and the thickness was about 1 millim.

It seems probable that a thin slice of bismuth, properly prepared and mounted, will come to be a valuable instrument for measuring the intensity of strong magnetic fields.

Antimony.

The powerful effect observed in bismuth suggested an examination of antimony. A slice of this metal, much like the slice of bismuth just mentioned, was prepared. The cross suffered in shape a good deal in making the adjustment. The final average thickness in the central portion was estimated at 1·2 millim.

* *Acc. dei Lincei Transunti*, June 1883.

Date.	Temp.	M.	C.	R. P.
1884, July 26th.	21.8	1696	.1150	$+114100 \times 10^{-15}$
Aug. 8th.	21.1	1638	.1104	+117300 „
Aug. 8th.	3	1638	.1104	+123900 „

The two trials of August 8th were not entirely independent of each other, as only one test of the intensity of the magnetic field and of the sensitiveness of the Thomson galvanometer was made during the two trials of that date. The results as they stand indicate an increase of the rotational power with fall of temperature ; but the experiments were two hasty to justify a conclusion upon the matter.

Several experiments have resulted from the criticisms or suggestions of other investigators.

Mr. Bidwell's Theory.

This has been so recently and so widely published that it is hardly necessary for me to state it in detail.

In 'Science' (March 28th, 1884) I replied very briefly to Mr. Bidwell's first paper, stating that I found the transverse current in a strip of soft steel to be in the same direction when the strip was fastened to the supporting plate by a clamp across the middle of the strip only, as when it was fastened by means of clamps at its ends only.

Mr. Bidwell has, in his second paper (Phil. Mag. April 1884), described an experiment which he appears to consider conclusive against the previously accepted view of the "rotational effect." In brief he obtains what he calls a "reversal" of the transverse effect in gold, using a strip of that metal having two narrow longitudinal slits lying on the same straight line and nearly meeting in the centre of the strip.

I shall not undertake here an extended discussion of this interesting experiment of Mr. Bidwell. My view of the matter is in substance as follows :—

Let l in fig. 3 represent an equipotential line in a strip of metal through which an electric current is flowing, the metal being in a normal condition.

In fig. 4 l represents the corresponding equipotential line when the metal strip is acted upon by a powerful magnetic force in a direction perpendicular to the plane of the paper.

If, now, a and b be connected by means of a wire, a current will flow from a to b through the wire. If a' and b' be con-

nected in the same way, a weaker current will flow from a' to b' through the wire.

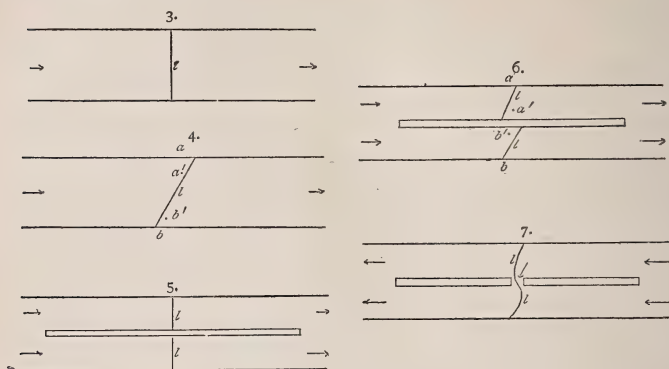


Fig. 5 represents a metal strip, having along its middle a slit longer than the diameter of the magnetic poles, between which the slit is placed, l and l' being corresponding equipotential lines when the strip is in its normal unstressed condition.

Let a magnetic force act as before. Each of the lines l , l' in fig. 5 is rotated about its centre through the same angle as l in figs. 3 and 4. The result is shown in fig. 6.

If now a and b be connected by means of a wire a current will flow from a to b through the wire. If a' and b' be connected in the same way a current will flow from b' to a' through the wire.

To get Mr. Bidwell's case, imagine a narrow bridge of metal to be left crossing the slit between a' and b' . Such a bridge would serve as a shunt to the wire which we have imagined connecting a' and b' , but would not reverse the relative potentials of these two points. The lines l , l' , as Sir Wm. Thomson pointed out at the Philadelphia meeting, would now take a form something like that shown in fig. 7.

As the bridge across the slit becomes wider such lines as l will become more nearly straight, till, finally, the condition of things represented in fig. 4 will be practically reproduced. On the other hand, if the slit in fig. 5 were short compared with the width of the poles of the magnet, a reversal such as Mr. Bidwell detected should not occur.

Mr. Tomlinson's Suggestion.

In the 'Philosophical Magazine' for May 1884, Mr. Tomlinson stated that certain relations which he had discovered

indicated that the rotational effect in nickel should be greater at low than at high temperatures. He therefore called in question the conclusion to the contrary which I had drawn from certain hasty and very rough experiments made before the use of the water-tank gave me the means of easily controlling the temperature of the metal under examination. I have therefore recently taken up the matter again. I used the same piece of nickel with which the former test was made (Phil. Mag. Sept. 1881, p. 163); but the strip was now so much damaged while being fastened upon glass, that it was necessary to cut its width down a good deal in order to obtain the "arms" for the side connections. The following Table shows the results obtained :—

No. of Exp.	Date.	Temp.	M.	C.	R. P.
1.	July 28, 1884.	22	1717	·03074	14940×10^{-15} .
2.	" 28	4·9	1703	·03135	13760 "
3.	" 29	20·5	1652	·03140	14710 "
4.	" 30	2·2	1636	·03123	12810 "
5.	" 30	21·2	1629	·03121	15060 "
6.	Aug. 1	2·8	1671	·03137	13180 "
7.	" 4	26·1	1691	·03140	15110 "
8.	" 4	4·1	1627	·03112	13220 "

Combining the 1st, 3rd, 5th, and 7th, we have

Temp.	R. P.
22°·5	14960×10^{-15} .

From the 2nd, 4th, 6th, and 8th we have

3°·5.	13240×10^{-15} .
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Decrease for 1° fall of temperature = approximately ·6 per cent.

It will be noticed that the results obtained at low temperatures do not accord so well as those at high temperatures. This fact is in all probability due to the difficulty experienced in preventing slight fluctuations of temperature while the colder water was flowing through the tank. Irregularity in the flow of this water causes disturbing thermo-electric currents.

The quantity called R. P. in this paper corresponds to the $\frac{E'}{V}$ of my 1881 paper divided by the F. of that paper. Calculating the R. P. of nickel from the 188 experiments, I find it to be about 12700×10^{-15} , for a field of intensity 1660.

In looking about for an explanation of this disagreement, I found that the thickness of the strip had been called $\cdot 00104$ cm. in 1881, and $\cdot 0012$ cm. in the later experiments. The former estimate was obtained by weight, the latter by measurement with calipers. The difference accounts well enough for the different values obtained for the R. P. Which estimate of the thickness is the more reliable I will not undertake to decide.

Summary.

$10^{15} \times \text{R.P. at } 20^\circ \text{ C.}$

Copper -520

Zinc $+820$

Four alloys of copper and zinc were tested. If a series be made beginning with zinc and ending with copper, the four alloys being ranged between according to composition, the series thus formed will be the same as that obtained by ranging these metals and alloys according to the algebraic magnitude of their R. P.'s. In the alloys, however, the R. P.'s are algebraically somewhat nearer that of copper than might be expected from the composition.

Iron $+7850$.

R. P. nearly constant through wide range of magnetic intensity, but apparently decreasing slightly at high intensities.

No ascertained *permanent* effect.

Steel, *soft* $+12060$.

Permanent effect about 5 per cent. of temporary effect.

Steel, *tempered* $+33000$.

Permanent effect about $1\frac{1}{2}$ per cent. of temporary effect.

Gold (no new test of magnitude of R. P.) -660

No ascertained permanent effect.

Cobalt $+2460$.

Nickel -14740 .

Bismuth -8580000 .

Antimony $+114000$.

A fall of 1° C. in temperature causes

in the R. P. of Iron, a fall of $\frac{2}{3}$ per cent. approx.

Steel, *soft*, " $\frac{1}{3}$ "

Steel, *temp'd*, " $\frac{1}{3}$ "

Cobalt, " 1 "

Nickel, " $\frac{2}{3}$ "

Non-magnetic metals, apparently a small increase.

Appendix.

At the Philadelphia meeting of the American Association I stated that I had compared the behaviour of a strip of soft steel cemented to a plate of glass with that of a similar strip fastened to a plate by means of a clamp. In preparing the matter for publication, however, I found that the test had not been made so carefully as was desirable; and I have therefore just repeated the experiment with the assistance of Mr. W. A. Stone, of the Harvard class of 1886.

I shall call the cemented strip A. It has been already mentioned as No. 1 in the preceding article. The clamped strip I shall call B. Both strips were cut from the same sheet of soft steel. Each is about 1.1 cm. wide and 3.2 cm. long between the terminals of brass. A is about .011 cm. thick and possibly a few per cent. thicker than B. A suffered rather more than B in the process of adjustment (fig. 1).

A was not only fastened to its plate with the cement of beeswax and resin, but was imbedded in the cement, the latter covering it with a layer, probably a millimetre or more in thickness. The plate was not placed in water for this test. The bearing of the clamp which fastened B to its plate was of wood, possibly a millimetre wide, and extended nearly from arm to arm of the strip. To prevent any very great bending, each end of the strip was loosely tied to the plate with a piece of twine. Otherwise the strip was free and was exposed to the air.

The measurements recorded below were made between five and six o'clock, January 17th. There was an interval of one minute between successive readings of the Thomson galvanometer. No measurement of the intensity of the magnetic field was made. It is assumed that this intensity during the one trial of A, which was made between the two trials of B, was equal to the mean intensity during these two trials.

The signs + and - refer to the direction of magnetization, R and L to direction from the zero point of the Thomson galvanometer-scale. The zero position of the index in these experiments was a considerable distance to the right of the zero of the scale. The tangent-galvanometer measured the direct current through the strip.

Thomson Galv.				Tangent Galv.		
				74.0		
Strip B	{	+	—	73.9		
		53	R	0	73.7	
		52	„	3	R	73.7
		51	„			73.6
	<hr/>			<hr/>		
		52	—	1.5 = 50.5	73.8, tan. = 3.44	

			74.9
			74.8
Strip A	{	43 R	74.7
		43 „	74.7
		43 „	74.6
		$\frac{43}{43} + \frac{8.0}{6.5} = 50.2$	$\frac{74.7}{74.7}, \tan. = 3.66$
			73.7
			73.6
Strip B	{	54.5 R	73.5
		53.0 „	73.4
		52.5 „	73.3
		$\frac{53.3}{53.3} - \frac{7.2}{7.2} = 46.1$	$\frac{73.5}{73.5}, \tan. = 3.38$

From 2nd we have

$$A. \quad 50.2 \div 3.66 = 13.7.$$

From 1st and 3rd

$$B. \quad \frac{50.5 + 46.1}{2} \div \frac{3.44 + 3.38}{2} = 14.2.$$

Of course absolute agreement in so rough a test was not to be expected. The difference of about 3 per cent. here observed might easily be accounted for. I think Mr. Bidwell will admit that his theory would have predicted a different result of the comparison.

Cambridge, U.S., January 19, 1885.

XLVII. *On the Structure of Mechanical Models illustrating some Properties of the Æther.* By Prof. GEORGE FRANCIS FITZGERALD, F.R.S.*

THE elements of which the model is constructed consist of pairs of wheels so geared together that when one of them rotates it causes the second to rotate in the same direction. The simplest way of effecting this is to connect them by a band, and this is sufficient for a one-dimensional model. Such a model may be constructed by fixing a number of wheels with their axes parallel and at right angles to a plane, and connecting each wheel with its neighbours by elastic bands. This represents a nonconducting region of the æther. A perfectly conducting region is one in which there are no bands, and a partially conducting region would be represented by the bands

* Communicated by the Physical Society : read March 28, 1885.

slipping more or less. A short description of how electrostatic, electrokinetic, and luminiferous phenomena are illustrated by such a one-dimensional model will be clearer than the corresponding description of the tridimensional model, the structure of which is the special purpose of this paper.

As an illustration of an electrostatic phenomenon, consider two conducting regions separated by a nonconducting region everywhere except along one line where the bands are removed. If anywhere in this line a rotation in opposite directions be communicated to the wheels that abut on it, then all the wheels in the nonconducting region will be turned more or less. If anywhere two neighbouring wheels turn equally, there is no straining of the band connecting them; but if one turn more than the other, the connecting band is strained and one side becomes tight and the other loose. Now it will be found in the case considered that all the bands are strained, and that all the tight sides are turned towards one conductor and all the loose sides towards the other. This represents the charging of the two conductors in opposite ways. The strain of the bands in any element of the medium represents the polarization of the element, and the line joining the tight and loose sides is the direction of polarization or of electric displacement. The energy of the system is in the form of this straining of the bands, which produces stresses tending to restore the unstrained condition. With a given strain at their respective surfaces, there would be more elements involved and more energy in the medium when the conductors are far apart than when near, showing that if we could represent in any way the fact that conductors can move through the æther there would be forces tending to produce this motion, or, in other words, there would be attraction between these oppositely electrified bodies. As, however, the model does not illustrate the connection between matter and æther, neither this nor magnetic attractions are represented, nor have electromotive forces such as exist in cells been represented. If the forces that have been supposed to turn the wheels along the conducting line connecting the two conducting regions cease to act, the state of strain will disappear, and what represents an electric discharge along this line will take place. All along this line, during the discharge, the wheels at opposite sides will be rotating in opposite directions; so that this is what represents an electric current at any point. It is the same as an electric displacement; and in a nonconductor such opposite rotation is resisted by the stresses in the band, but in a conductor it may take place to any extent. During the discharge the whole of the nonconducting region is full of rotating wheels, and their axes of rotation are at

right angles to the direction of discharge. Their velocity of rotation evidently represents the magnetic force accompanying the discharge, and the momentum of the wheels represents the electrokinetic momentum of the current, *i.e.* its self-induction. This is further illustrated by this, that if the frictional resistance be small enough, this momentum will carry the wheels beyond their positions of equilibrium, and there will result an oscillating discharge, such as occurs when an electric condenser is discharged through a sufficiently small resistance. If we suppose a certain amount of frictional resistance at any point along the line of discharge, we may see that the energy expended on friction is conveyed to the place by the bands in the surrounding nonconductor and comes in at the side of the conductor, in accordance with Prof. Poynting's theorem as to the direction of the flow of energy in an electrodynamic system.

The mutual induction of two circuits may also be illustrated by the model. Sufficient has been explained, however, to show how electrostatic and electrokinetic phenomena are represented on the model. If a sudden movement of rotation be communicated to any set of wheels, it is evident that inertia will prevent their neighbours being instantaneously turned, while the connecting bands will be strained. Rotation will, however, be communicated to the neighbouring wheels, and from them to their neighbours, by a process which is a species of wave-propagation. If we consider the nature of the disturbance which is thus propagated, we see that it consists in a rotation whose axis is at right angles to the direction of propagation, and of a polarization of the bands which is at right angles both to the axis of rotation and to the direction of wave-propagation. These are respectively a magnetic and an electric displacement, which are at right angles to one another and to the direction of wave-propagation. This is exactly in accordance with Maxwell's electromagnetic theory of light-propagation. It is thus seen how the same model that can represent electrostatic and electromagnetic phenomena also illustrates luminiferous phenomena by its small oscillations.

If we try to produce a tridimensional model by means of wheels geared together by bands, we are met by the following difficulty. The energy of the model we have been considering may be represented in the following way:—Let ζ represent the angular rotation of any wheel from a given position; then the kinetic energy of an element will evidently be proportional to $\dot{\zeta}^2$, while the potential energy of an element, depending as it does on the difference of rotation of neigh-

bouring wheels, will be proportional to $\left(\frac{d\zeta}{dx}\right)^2 + \left(\frac{d\zeta}{dy}\right)^2$. If we were to build up a tridimensional model by simply putting together three such systems of wheels in three rectangular planes, the kinetic energy of an element of the model would be proportional to

$$\xi^2 + \eta^2 + \zeta^2,$$

but its potential energy would be simply

$$\left|\frac{d\zeta}{dx}\right|^2 + \left|\frac{d\zeta}{dy}\right|^2 + \left|\frac{d\eta}{dz}\right|^2 + \left|\frac{d\eta}{dx}\right|^2 + \left|\frac{d\xi}{dy}\right|^2 + \left|\frac{d\xi}{dz}\right|^2,$$

instead of

$$\left(\frac{d\zeta}{dy} - \frac{d\eta}{dz}\right)^2 + \left(\frac{d\xi}{dz} - \frac{d\zeta}{dx}\right)^2 + \left(\frac{d\eta}{dx} - \frac{d\xi}{dy}\right)^2,$$

which is what it should be if it is to represent the æther.

The electrostatic and electrokinetic energies of an element of the æther may be expressed in this form by assuming ξ , η , and ζ such that their velocities $\dot{\xi}$, $\dot{\eta}$, and $\dot{\zeta}$ are α , β , and γ , the components of the magnetic force at the element. Now simple band-gearing will not enable us to arrange that no

straining shall result, when, for instance, $\frac{d\eta}{dx} = \frac{d\xi}{dy}$, but this

may be accomplished by the following arrangement:—Each element of the æther is to be represented by a cube, on each edge of which there is a paddle-wheel. Thus on any face of the cube there will be four paddle-wheels. Now, if any opposite pair of paddle-wheels on a face rotate by different amounts, they will tend to pump any liquid in which the whole element is immersed into or out of the cube, and if the sides of the cube be elastic there will be a stress which will tend to stop this differential rotation of the wheels. If, however, the other pair also rotate by different amounts they may undo what the first pair do; and thus the stress will depend on the difference between the differential rotations of these opposite pairs of

wheels, *i. e.* on $\frac{d\eta}{dx} - \frac{d\xi}{dy}$.

In order that these four wheels may not similarly work with any other wheel in the cube it is necessary to place diaphragms, cutting the cube into six cells, each a pyramid standing on a face of the cube. These must be so made that liquid may not be able to pass from one cell to another through the diaphragm, nor beside the paddle-wheels. In order actually to effect this, the floats on the paddle-wheels would have to be drawn down while passing the diaphragm (of

course these mechanical details could hardly be carried out so as to work with sufficiently little friction for the working of any actual model to approximate sufficiently closely to that of the æther for it to be worth while attempting to construct it). The faces of the cube should be filled up with diaphragms past which the paddle-wheels should pump liquid, and whose elasticity should be the means of storing electrostatic energy in the medium. It may be worth while pointing out some of the results of having shown that the energy of a disturbance of this medium would be represented by the same equations as those Maxwell has shown to hold for the æther. The most complicated results follow from supposing the faces of the cubes, of which the medium is constructed, to have different elasticities. Such a structure represents a crystalline medium. Its vibrations would be propagated according to the laws of propagation of light in crystalline media. The wave-surface would be Fresnel's wave-surface, and it would exhibit conical refraction. If the cubes were twisted, the structure would be like that of quartz or other substances that rotate the plane of polarization of light. In order to represent the rotation of the plane of polarization of light by magnetism, it would be necessary to introduce some mechanism connecting this æther with matter. That this would be required is evident from the consideration that no amount of inherent rotation of these wheels would alter the plane of polarization of a vibration transmitted by them. Now, although I am not prepared to suggest any actual method of connection, it may be worth while pointing out two different ways in which it may work. In the first place the matter may be in rotation when subject to magnetic force ; and it may be connected with the æther in such a way that the direction of its axis and rotation is altered by the vibrations passing in the æther. It would then react on the æther in such a way as to rotate the plane of polarization of the wave. In the second place the matter might only act as a link connecting æther elements rotating in rectangular directions in such a way that the rotation of one element altered the axis of rotation of a rectangular element. The reaction of this latter on the former would then rotate the plane of polarization of a wave propagated parallel to the latter. The first of these connections is that in accordance with Maxwell's theory as to the connection between the æther and matter that he introduces in his 'Electricity and Magnetism,' in order to explain the rotation of the plane of polarization of light by magnetism ; while the second is the one that I supposed in my paper "On the Electromagnetic Theory of the Reflection and Refraction of Light" (R. S. Trans. 1880).

I need hardly say, in conclusion, that I do not in the least intend to convey the impression that the actual structure of the æther is a bit like what I have described. What physicists ought to look for is such a mode of motion in space as will confer upon it the properties required in order that it may exhibit electromagnetic phenomena. Such a mode of motion would be a real explanation of these phenomena. I have only given a description of them.

I think, however, that it is worth while considering these models, because in them the disturbance which represents light is not the same as the vibrations of an elastic jelly, for what represents an electric displacement is a change of structure of an element, and not a displacement of the element; and it seems almost certain that, notwithstanding the very high authority which seems to support the view that the æther is *like* an elastic jelly, nevertheless its vibrations are much more of the nature of alterations in structure than of displacements.

XLVIII. *On the Theory of Illumination in a Fog.*

By Lord RAYLEIGH, F.R.S.*

AS a step towards a better understanding of the action of fog upon light, it seems desirable to investigate what the phenomena would be in the simplest case that can be proposed. For this purpose we may consider the atmosphere and the material composing the fog to be *absolutely transparent*, and also make abstraction from the influence of obstacles, among which must be included the ground itself.

Conceive a small source of radiation, *e. g.* an incandescent carbon filament, to be surrounded by a spherical cloud, of uniform density, or at any rate symmetrically disposed round the source, outside of which the atmosphere is clear. Since by hypothesis there is no absorption, whatever radiation is emitted by the source passes outward through the external surface of the cloud. The effect of the cloud is to cause diffusion, *i. e.* to spread the rays passing through any small area of the surface (which in the absence of the cloud would be limited to a small solid angle) more or less uniformly over the complete hemisphere.

Whether the total radiation passing outwards through the small area on the external surface of the cloud is affected by the existence of the cloud depends upon the circumstances of the case. If it be laid down that the total emission of energy from the source is given, then the presence of the cloud

* Communicated by the Physical Society: read April 25, 1885.

makes no difference in respect of the energy passing any element of the spherical area. But this supposition does not correspond to a constant temperature of the source, in consequence of the energy received back from the cloud by reflection. To keep the total emission of energy constant, we should have to suppose a rise of temperature increasing indefinitely with the size and density of the cloud.

Let us now suppose that the region under consideration is bounded upon all sides by a distant envelope of perfect reflecting-power. Then, whatever the density of the clouds which may wholly or partially occupy the enclosure, we know, by the second law of thermodynamics, that at every internal point there is radiation in every direction of the full amount corresponding to the temperature of the source. In one sense this conclusion holds good, even although the matter composing the cloud has the power of absorption. But in that case equilibrium would not be attained until the clouds themselves to the remotest parts had acquired the temperature of the source; whereas under the supposition of perfect transparency the temperature of the cloud is a matter of indifference; and equilibrium is attained in a time dependent upon that required by light to traverse the enclosure. So far we have made no supposition as to the distribution of the cloud; but we will now imagine a layer of such thickness as to allow only a very small fraction of the incident radiation to penetrate it, to line the interior of the reflecting envelope. This layer itself plays the part of a practically perfect reflector; and it is not difficult to see that the reflecting envelope hitherto conceived to lie beyond it may be removed without interfering with the state of things on the inner side of the layer of cloud. We thus arrive at the rather startling conclusion that at any distance from the source, and whatever the distribution of clouds, there is always in every direction the full radiation due to the temperature of the source, provided only that there lie outside a complete shell of cloud sufficiently thick to be impervious. And this state of things is maintained without (on the whole) emission of energy from the source.

Even if the material composing the cloud possesses absorbing-power for some kinds of radiation, *e. g.* for dark radiation, but is perfectly transparent to other kinds, *e. g.* luminous radiation, the general theorem holds good as respects the latter kinds; so that in the case supposed the light would still be everywhere the same as in a clear enclosure whose walls have throughout the same luminosity as the source. But in order to compensate the absorption of dark rays, the source must now be supplied with energy.

Some of the principles here enunciated have an acoustical as well as an optical application, and indeed first occurred to me some years ago in connection with Prof. Tyndall's investigations upon fog-signals. The effect of "acoustic clouds" analogous to fog (and unattended with absorption of energy), might be very different upon the report of a gun and upon the sustained sound of a siren, the latter being reinforced by reflection from the acoustic fog.

The theory presented in the present paper may be illustrated by the known solution of the comparatively simple problem of a pile of transparent plates*. If ρ denote the proportion of the incident light reflected at a single surface, then the proportion reflected $\phi(m)$, and transmitted $\psi(m)$, by a pile of m plates is given by

$$\frac{\phi(m)}{2m\rho} = \frac{\psi(m)}{1-\rho} = \frac{1}{1+(2m-1)\rho}.$$

From these expressions it is evident that, however small ρ may be, *i. e.* however feeble the reflection at a single surface, we have only to suppose m large enough in order that the reflection may be as complete, and the transmission as small, as we please. Such a pile may, under ordinary conditions, be regarded as impervious.

But now suppose that after passing the pile of m plates, the light is incident upon a second pile of n plates, and consider the intensity between the two piles, the original intensity being unity, as before. For the intensity of the light travelling in the original direction we have

$$\begin{aligned} &\psi(m) + \psi(m) \cdot \phi(n) \cdot \phi(m) + \psi(m) \cdot \{\phi(n) \cdot \phi(m)\}^2 \\ &+ \psi(m) \cdot \{\phi(n) \cdot \phi(m)\}^3 + \dots; \end{aligned}$$

or on summation of the geometric series,

$$\frac{\psi(m)}{1 - \phi(n) \cdot \phi(m)}.$$

If we introduce the values of ϕ and ψ in terms of m, n, ρ , this becomes

$$\frac{2n\rho + 1 - \rho}{2(m+n)\rho + 1 - \rho}.$$

In like manner, for the light going the other way we have

$$\frac{\phi(m) \cdot \phi(n)}{1 - \phi(m) \cdot \phi(n)};$$

* Stokes, Proc. Roy. Soc. vol. xi. p. 545 (1862).

or in terms of m, n, ρ ,

$$\frac{2np}{2(m+n)\rho + 1 - \rho}.$$

When m and n are great, both expressions reduce to $n/(m+n)$; so that the light passing in the two directions is equally bright. Moreover, and this is the point to be especially noticed, however great m may be—that is, however impervious the first pile is, the light between the two piles may be made to approach the original light in brightness as nearly as we please, by sufficiently increasing the number of plates in the second pile; that is, the light between the piles may be made to be the same as if the first pile were removed. From this example we may understand more clearly how a very small quantity of light penetrating directly may be beaten backwards and forwards, as between two reflectors, until the original intensity is recovered.

XLIX. *A Monochromatic Telescope, with Application to Photometry.* By LORD RAYLEIGH, F.R.S.*

THE purpose of this instrument is to exhibit external objects as they would be seen either with the naked eye, or through a telescope, if lighted with approximately monochromatic light; that is, to do more perfectly what is done roughly by a coloured glass.

The arrangement is not new, though I am not aware that it has ever been described. In 1870 I employed it for determinations of absorption, and, if my memory serves me right, I heard soon afterwards from Clerk-Maxwell that he also had used it. It is, indeed, a very slight modification of Maxwell's colour-box.

In the ordinary form of that instrument, white light admitted through a slit is rendered parallel by a collimating lens, dispersed by flint-glass prisms, and then brought to a focus at a screen, upon which accordingly a pure spectrum is formed. This screen is perforated by a second slit, immediately behind which the observer places his eye. It is evident that the light passing the aperture is approximately monochromatic, so that the observer, if he focuses his eye suitably, will see the prism illuminated with this kind of light. The only addition now required to convert the instrument into a monochromatic telescope is a lens placed just within the first slit, of such power as to throw an image of external objects

* Communicated by the Physical Society: read April 25, 1885.

upon the prism or diaphragm upon which the eye is focused. If desired, an eye-lens may be placed at the second slit; but this is not generally needed.

In the present instrument a direct-vision dispersing prism is used, so that the optical parts can be all disposed in a narrow box of nearly 3 feet in length. The lenses are all single lenses, and work sufficiently well. The slits are of such width that either coincides with the image of the other, and their relative position is so chosen that the mean refrangibility of the light is that corresponding to sodium. Objects seen through the instrument thus appear as if lighted by a sodium flame.

The principal object which I had in view in the construction of the instrument now exhibited was to see whether it could be made of service in the comparison of compound lights of somewhat different colours—a problem just now attracting attention in connection with electric lighting. It is scarcely necessary to say that a comparison of this kind is physically incomplete unless it extends to all the spectral components separately; but for commercial purposes such an extended comparison is too complicated, and indeed useless. Determinations at two points of the spectrum, as proposed by Capt. Abney, would certainly suffice for ordinary purposes; and in view of the convenience of expressing the result by a single number, it is not unlikely that people practically concerned in these matters will content themselves with a comparison at one point. It seems desirable that some convention should be arrived at without much further delay; so that two lights should be considered to be commercially equal, if they have the same intensity of, *e. g.*, sodium or of thallium light. It will be understood that such a mode of estimation assumes that the intensity varies along the spectrum in a gradual manner; and this consideration may tell against the use of the sodium light as a standard, inasmuch as the component in question often predominates unduly in candle-flames from the actual presence of sodium.

Whatever choice be made, an instrument like the present may be employed to make the desired selection; and it is applicable to any photometric arrangement. For my own experiments I have used the shadow-method, and find it possible to compare any bright sources, however different in colour. The only difficulty arises from the necessary enfeeblement of the light by selection, and this practically precludes observation with standard candles. With gas-flames and glow-lamps the light is sufficient.

L. *Note on a slight Error in the customary Specification of Thermo-electric Current-direction, and a Query with regard to a point in Thermodynamics.* By OLIVER J. LODGE, D.Sc., Professor of Physics in University College, Liverpool*.

[Plate III.]

IN a paper written for the British Association at Montreal, "On the Seat of the Electromotive Forces in the Voltaic Cell," printed in the Philosophical Magazine for March, April, and May this year, there occurs the following footnote to section 23 (Phil. Mag. May 1885, vol. xix. p. 355):—

"It is always easy to tell from thermoelectric data which way the force acts at a junction; but it is not always the same way as the current flows, by any means. A current often flows against the E.M.F. even at a hot junction, and it may flow against the force at *both* junctions. This is the case, for instance, in a copper-iron circuit with one junction above 275° , and the other below that temperature by a greater amount. It is customary to say that the current flows across a hot junction from the metal of higher to the metal of lower thermoelectric value: this is not necessarily true. The safe statement is to say that the E.M.F. acts from high to low thermoelectric value at either junction."

A short statement like this is apt to be buried in a long paper on other matters, and though not of any high importance, it is always worth while to keep as clear as possible on all elementary matters, if only for the sake of one's students. The point is perhaps most easily illustrated by considering special cases; but I may first briefly explain my general point of view in case there may be some objection to it.

It is well known that the whole E.M.F. of a thermoelectric circuit is obtainable by integrating, all round, a certain function of the temperature and of the metals in circuit, which may be called the Peltier function, Π ,

$$E = \int \frac{J\Pi d\theta}{\theta},$$

where the θ stands for absolute temperature. On Tait's assumption that what Thomson called "the specific heat of electricity in a metal" is proportional to θ —equal, say, to $k_a\theta$ for metal A, and to $k_b\theta$ for metal B—the value of Π is

$$\Pi = (k_a - k_b)\theta(\theta_0 - \theta),$$

where θ_0 is a constant to be determined by experiment, just as k_a and k_b are to be determined.

* Communicated by the Author.

From these equations the value of the total E.M.F. of the simple circuit, with its two junctions at temperature θ_1 and θ_2 , is obviously

$$E = (k_a - k_b)(\theta_1 - \theta_2) \left\{ \theta_0 - \frac{1}{2}(\theta_1 + \theta_2) \right\};$$

a well-known formula, first established empirically by Avenarius as far as the variable part is concerned.

Now although Π , the Peltier coefficient, can thus be regarded as a function to be integrated all round the circuit, it is possible also to regard it as localized—part of it constituting an E.M.F. residing at the junctions, and part of it having its abode wherever a slope of temperature occurs in either metal. And, indeed, the generation and destruction of heat at the junctions, observable when a current flows round the circuit and known as the Peltier effect, together with the apparent convection of heat by electricity discovered by Sir William Thomson, and which in 1876* I ventured to call the Thomson effect, compel us to picture to ourselves an E.M.F. at each junction, which may be called specifically *Peltier forces* and be denoted by Π_1 and Π_2 , and another E.M.F. in each metal wherever the temperature slopes; the total force in the metal A between the temperatures θ_1 and θ_2 being denoted by Θ_a , that in the other metal by Θ_b , and both being called *Thomson forces*.

The Peltier effect and the Thomson effect depend on a current passing; the existence of the *forces* is independent of such an accident. The Thomson force depends for its existence upon inequality of temperature; the Peltier force does not, it varies only with absolute temperature and nature of metals. Equality of temperature throughout the circuit abolishes the Thomson forces, but only renders equal the Peltier ones, their continued existence being provable by producing the Peltier effect. The resultant E.M.F. of the whole simple circuit, with junctions at θ_1 and θ_2 , is

$$E = \Pi_1 - \Pi_2 + \Theta_a - \Theta_b;$$

whence, from the preceding equations, we get

$$\Pi_1 = (k_a - k_b)\theta_1(\theta_0 - \theta_1),$$

$$\Pi_2 = (k_a - k_b)\theta_2(\theta_0 - \theta_2),$$

$$\Theta_a = k_a(\theta_1^2 - \theta_2^2),$$

$$\Theta_b = k_b(\theta_1^2 - \theta_2^2),$$

where the k 's may quite easily be negative in certain metals.

By these formulæ the tables of Peltier and Thomson contact series, given in section 23 of the paper† above referred to, were

* Phil. Mag. [5] Dec. Suppl. vol. ii. p. 534.

† Phil. Mag. May 1885, pp. 354, 355.

reckoned from Tait's experimental data of thermoelectric powers: the term "thermoelectric power" being defined, for a circuit of two metals, A and B, thus:—

$$P_a - P_b = \frac{E}{\theta_1 - \theta_2}.$$

It is now easy to follow out the changes in magnitude and sign of the four forces Π_1 , Π_2 , Θ_a , and Θ_b for a circuit of any two metals as their junctions are varied in temperature; and the diagrams (Plate III.), illustrative of twelve typical cases observable with an iron-copper circuit, are not without interest.

The Peltier force acts from copper to iron for all temperatures below the neutral point 275°C. ; at this point it vanishes, and above this point it is reversed: in other words, copper is the metal of higher thermoelectric power below 275° , but iron is, above. The Thomson force happens to act from hot to cold in iron, and from cold to hot in copper, always; and it increases uniformly with the difference of the squares of the absolute end-temperatures.

The temperature of each junction is indicated on the figures in Centigrade degrees. The arrows indicate the direction of the electromotive forces at the different places, and numbers attached to the arrows show their magnitude. The central arrow represents the resultant E.M.F., or current, in magnitude and direction. The unit in which all the E.M.F.s are specified is the ten-thousandth of a volt.

It will be observed that in cases 4 and 7 the current flows against the force at the hot junction; and that in cases 6 and 9 it flows with the force at the cold junction, being helped on thereby. The common statement that the current flows from a metal of higher to a metal of lower thermoelectric value across the hot junction is thus by no means necessarily true. Its truth depends upon circumstances. It is not even true to say that the self-generated current necessarily cools the hot junction and warms the cold one. It may warm both; it may cool both. It may even, in some peculiar cases, cool the cold junction and warm the hot: see below.

These diagrams, though they express nothing but what is well known, the numbers being calculated from Tait's table of thermoelectric values as given in any textbook, yet are interesting from the point of view of the second law of thermodynamics, being some of them apparent exceptions to, or contradictions of, some statements of that law.

It will be observed that the current usually flows with the force at the hot junction, and therefore cools it; but it may

flow against this force, thus *increasing the temperature of the hottest part of the circuit.*

Again, the current usually flows against the force at the cold junction, and therefore warms it; but it may flow with this force, thus *lowering the temperature of the coolest portion of the circuit.*

It would be curious to obtain a case where the current flowed *with* the force at the cold junction and *against* that at the hot, thus carrying heat from the coldest to the hottest parts of the system. But, so long as the lines of a thermoelectric diagram are straight, this is not possible; as may be seen by proving the following criteria:—

Calling the temperature of the hot junction t_1 ,
 „ „ „ cold junction t_2 ,
 „ „ „ neutral point t_0 ,
 and denoting by t the average temperature $\frac{1}{2}(t_1 + t_2)$, we can say:—

The current flows against the force at the hot junction only when the neutral point lies between the highest and the average temperatures;

i. e. when

$$t_1 > t_0 > t.$$

The current flows with the force at the cold junction only when the neutral point lies between the lowest and average temperatures;

i. e. when

$$t > t_0 > t_2.$$

These conditions, though either of them is easily possible, are mutually incompatible; hence *both* cases cannot occur at once. In other words, if the current flow against the force at the hot junction, it must flow against the force at the cold one too.

The same thing is also evident by considering a thermoelectric diagram, though perhaps not quite so easily. It will then, however, be further perceived that the impossibility under consideration is not a fundamental one depending on thermodynamic laws, but is a mere consequence of the experimental straightness of the thermoelectric lines; and that if for any metal the line is really curved*, as may well be the case, so that between it and another there are two neutral points, it is quite possible to arrange so that the current shall be assisted by the force at the cold junction, and opposed by the force

* Prof. Tait says that the line is strongly curved for nickel; and that an iron-nickel junction may have three neutral points (Tait's 'Heat,' p. 178).

at the hot. In other words, it is possible for the hot junction to be heated and the cold junction cooled, or for the initial inequality of temperature to be *increased* by the current excited by reason of that very inequality; unless the electric convection of heat along the metals reduces the inequality of temperature to a greater extent. But for this act of repa-
 ration there is no necessity; for instance, in the special cases considered, the current opposes the flow of heat by conduction both in iron and in copper; carrying cold with it, so to speak, in both cases. Thus the electric convection of heat, on the whole, tends to accentuate the difference of temperature in these cases. *Conduction* may reduce it, but in Thomson's theory conduction is carefully regarded as a non-essential concomitant.

Now how is it possible for the hottest part of a system to be automatically warmed, and for the coldest portion to be automatically cooled? Or, again, how can an automatically generated current be assisted by heat derived from the coldest part of a system, and be opposed by having to generate heat at the hottest part?

According to some statements of the second law of thermodynamics, such a result would be paradoxical or impossible. And yet the result is deduced by help of the second law itself:

the form of the law being, $\int \frac{dH}{\theta} = 0$ when taken round a circuit, or $\frac{dH}{\theta}$ is a complete differential of a temperature-function; dependent therefore on end-temperatures only, not on nature of metal or distribution of intermediate temperatures.

If H is the heat converted into electrical energy at any part of the circuit, and E the resultant total E.M.F., then no doubt, as in any reversible heat-engine, the work done per cycle

$$E\gamma = \int \frac{H d\theta}{\theta} \text{ taken round the circuit ;}$$

and the disappearance of heat exactly accounts for the electrical energy without having to fall back upon degradation by irreversible processes such as conduction. But the noteworthy thing is that, whereas usually heat is destroyed at the hotter parts and generated at the colder parts of a system, in this case heat may be generated at the hottest and destroyed at the coldest parts; the great bulk of the conversion of heat into electrical energy being carried on at intermediate temperatures. H is not a bit proportional to absolute temperature, as it is in a simple heat-engine; it may even be zero or negative, and may attain its maximum at any temperature, except indeed at the very lowest in the system.

I am sorry to say that I did not see this clearly at first, nor do I see it now as clearly as I should like, and I have accordingly wasted three days in an attempt to take account of conduction of heat, or at least of a certain minimum amount of it, as if it were an essential part of the process; the raising of heat-potential by the current in certain cases being only accountable, as it seemed to me, by reason of a definite and calculable amount of degradation by conduction; after the manner of a water-ram.

Liverpool, 8th May, 1885.

LI. *On Supersaturation of Salt-Solutions.* By W. W. J. NICOL, M.A., D.Sc., Lecturer on Chemistry, Mason College, Birmingham*.

“Le sulfate de soude dissous dans l’eau à des températures quelconques est anhydre. Il n’y a de solution sursaturée.”†

BEFORE I proceed to give an account of my experiments on this subject and the conclusions based on them, it is necessary to point out that there are two distinct kinds of supersaturation.

Supersaturation, in general, may be defined as—

The existence in solution of a larger quantity of salt than the water is normally capable of holding in solution at the temperature of experiment.

Or as clearly expressed by Mulder‡—

“Oververzadiging is: meer in oplossing hebben bij eene zekere temperatuur dan bij die temperatuur in oplossing kan overgaan, wanneer men begint met het vaste zout en dat behandelt met water bij die temperatuur . . . Oververzadiging is dus: behoud van een toestand, aan het zout gegeven door eene hoogere temperatuur.”

“Supersaturation is the holding in solution at a given temperature more salt than can be dissolved at that temperature, starting with the solid salt and treating it with water at that temperature. It is thus the possession of a condition which is conferred on a salt by a higher temperature.”

Supersaturation as thus defined is common to all salts without exception. But it will be seen on reflection that, as I have said, there are two kinds:—

1st. That occurring in *presence* of undissolved salt.

* Communicated by the Author, having been read before the Royal Society of Edinburgh, April 1885.

† Loewel, *Ann. d. Chim. et Phys.* [3] xlix. p. 51.

‡ *Bijdragen tot de Geschiedenis van het scheikundig gebonden Water* (Rotterdam, 1864).

2nd. That which manifests itself only in the *absence* of undissolved salt.

Of the above, the former occurs with all salts, hydrated or anhydrous in the solid form. Its existence is dependent solely on the fact that a finite time is necessary to permit of the establishment of equilibrium or saturation in a solution. When a hot saturated solution is cooled to the temperature of the air, the first portions of the excess of salt crystallize out at once, but more slowly as the excess becomes less and less, until when equilibrium is nearly perfect separation of solid salt is extremely slow. This has been commented on by Kremers under the name of "Inertia,"* and is a fertile source of error in determinations of solubility. I have myself detected traces of this form of supersaturation even in the case of solutions which have been prepared at a high temperature and, after cooling, agitated for twenty-four hours with the solid salt; the percentage dissolved was frequently slightly greater than in the case of solutions prepared from solid salt and water without previous heating†.

This is, I hope to show, true supersaturation; it is never well marked, and is not permanent, the excess of salt thus remaining in solution being exceedingly small, and becoming less and less with time.

The phenomenon usually termed supersaturation is the second of the two kinds I have distinguished, and is quite distinct from the former. It occurs only with hydrated salts, and is well marked and permanent. The amount of salt thus retained may be very large, and crystallization may, under certain conditions, be delayed indefinitely. It is manifested whenever a strong warm solution of a hydrated salt is allowed to cool out of contact with the air or in contact with air which has been heated or filtered through cotton-wool. In nearly all cases such solutions remain permanently liquid, depositing crystals on strong cooling, which dissolve again on the application of a gentle heat, such as that of the hand. No crystallization is caused by shaking or by the passage of electricity; but crystallization takes place at once on the addition of a crystal of the hydrated salt.

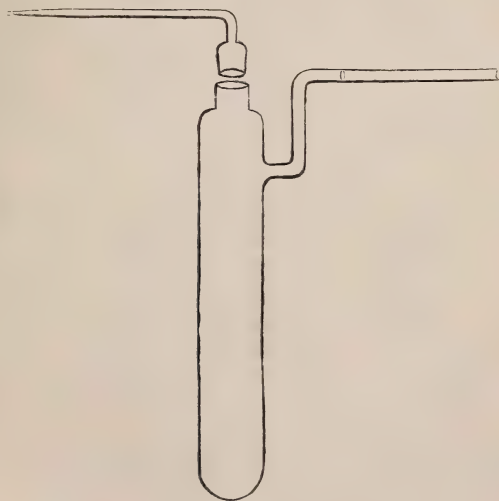
It is with such cases of supersaturation that this paper has to deal, and I hope to be able to show that, though a solution may deposit salt under the above conditions, yet it is not *really* supersaturated. I have confined myself strictly to the state of the solutions, and have no remarks to offer on the causes which bring about crystallization.

* Pogg. *Ann.* lxxxv. p. 41.

† Phil. Mag. June (Suppl.) 1884.

The question I have set myself is: What is a (so-called) supersaturated solution? And the answer I have obtained is a very simple, though at first sight strange one. It is a non-saturated or just-saturated solution of the anhydrous salt.

It is nearly three years since I made my first experiments on this subject, by determining the density of supersaturated solutions of sulphate and thiosulphate of sodium before and after crystallization; but at that time the results obtained had no meaning to me, and it is only recently that I have been able to interpret them. The *modus operandi* was as follows:—A quantity of salt was placed in a weighed specific-gravity bottle, which was again weighed. A quantity of water was added, and the whole reweighed; then some paraffin-oil of known density was added, and the bottle was placed in hot water till complete solution was effected. After cooling, the bottle was filled up with paraffin, the stopper inserted, and then placed in a constant-temperature bath for some time, and finally dried and weighed. When the stopper was removed, a minute crystal of the salt under examination, when dropped through the paraffin into the solution, produced instant crystallization. After three or four hours in the constant-temperature bath crystallization was assumed to be complete, and, after refilling with paraffin, the bottle was again weighed. From the data thus obtained it was easy to calculate the



density of the solution, and also that of the mixture of salt and saturated solution resulting from the crystallization. It

was found that separation of Na_2SO_4 10 aq. was attended by expansion, $\text{Na}_2\text{S}_2\text{O}_3$ 5 aq. by contraction; and it was this contradictory behaviour of the two salts that caused me to abandon the experiments for the time.

In more recent experiments I employed the apparatus in the figure, which is in many respects more accurate and more easily worked with than a bottle; at the same time I saw the futility of determining the density after crystallization: the whole secret was to be found in the solution before solidification. Expansion or contraction was a mere accident peculiar to the salt employed.

Table I. contains the results of all my experiments calculated for anhydrous salt-molecules per hundred water-molecules with the corresponding apparent molecular volumes $\left(\frac{r}{n}\right)$ of each salt-molecule.

TABLE I.

$n(\text{Na}_2\text{S}_2\text{O}_3) 100 \text{ H}_2\text{O}.$				
$t^\circ.$	$n.$	Density.	Mol. vol.	$\frac{\text{MV}-1800.}{n}.$
20	7.665*	1.38896	2167.84	47.99
"	9.216	1.43835	2263.80	50.33
"	10.057	1.46231	2317.54	51.46
"	11.071	1.49392	2357.70	52.01
"	11.679	1.50136	2428.03	53.77
"	12.132	1.50383	2471.56	55.35?
"	15.149	1.59006	2637.37	55.29
"	20.000†	1.67335	2964.20	58.21
$n(\text{Na}_2\text{SO}_4) 100 \text{ H}_2\text{O}.$				
20	.5	1.03466	1808.32	16.64
"	1.0	1.06744	1819.31	19.31
"	1.847*	1.11733	1845.77	24.71
"	3.193	1.19215	1890.18	28.25
"	4.442	1.25855	1931.40	29.58
"	4.729	1.26698	1969.35	31.87
"	6.244	1.34014	2004.71	32.79

* Saturated at 20° .

† Fused crystals.

In the case of sodium thiosulphate, the first solution determined was that saturated for the hydrated salt at $20^\circ \text{C}.$, and after that solutions of various strengths up to that consisting of the salt fused in its water of crystallization. With sodium sulphate the solutions ranged in strength from the half-molecule up to one containing more than six molecules of salt per

hundred water-molecules; this last is the strongest solution that can be conveniently worked with. In the case of both salts the molecular volume constantly increases with the concentration, very rapidly at first, and then more slowly, just as it does in the case of an ordinary solution of an anhydrous salt, such as potassium nitrate. The chief point of importance is the absence of any abrupt break above the ordinary saturation-point; there is no discontinuity, the steady increase is unbroken; there is nothing in the numbers for the molecular volumes to indicate a difference in the constitution of the solution. I ought to add that, owing to the unavoidable loss of water during the heating necessary for the solution of the salt, it is not to be expected that the density determinations should be as accurate as those of ordinary solutions; not much reliance can be placed, I fear, on the fourth figure; not by any means so much as on the fifth figure in other cases: the effect of this is to affect the molecular volume in the first decimal to the extent of ± 5 .

It would seem, then, that these solutions were simply solutions of the anhydrous salt which are yet unsaturated. I have already expressed my conviction that a salt in solution parts with its water of crystallization*, and attempted to prove it by experiment. If this were correct, and the supersaturated solution such as I conceived it to be, it followed that anhydrous sodium sulphate should dissolve in a so-called supersaturated solution; in other words, a solution saturated by contact with *crystals of a hydrated salt* should be able to dissolve that salt *when dehydrated*.

To test this a wide-mouthed bottle was nearly filled with crystals of $\text{Na}_2\text{S}_2\text{O}_3 \cdot 5 \text{ aq.}$, and placed in boiling water. The quantity of salt was nearly 60 grammes; a small, thin glass bulb containing less than a gramme of the dehydrated salt, and sealed, was also placed in the bottle. When the thiosulphate was completely fused, the stopper was put in, and, after cooling slightly, the bottle was placed in water at about 15°C . Thiosulphate of sodium fuses at 48° to 50°C ., yet the solution thus obtained was perfectly stable; no crystallization was caused by gentle shaking. By a smart blow of the bottle on the knee the small bulb was broken, and the dehydrated salt brought in contact with the solution; no crystallization took place; on the contrary, solution was complete in a few seconds. The experiment was repeated with from 4 to 4.5 grms. of $\text{Na}_2\text{S}_2\text{O}_3$ to 40 grms. of $\text{Na}_2\text{S}_2\text{O}_3 \cdot 5 \text{ aq.}$ In this case solution was not complete at the ordinary temperature, but was readily effected by heat. When the solu-

* Phil. Mag. Sept. 1884, p. 181.

tion thus obtained was cooled, crystals slowly separated out at the air temperature. Some crystals thus obtained were drained as completely as possible from the oily mother-liquor, and then removed and pressed between filter-paper; in spite of the draining they were found to be imbedded in a mass of the solidified hydrated salt. On heating, the original crystals did not change form, and remained nearly transparent. The total loss of water on the mixture was 17 per cent., corresponding to $\text{Na}_2\text{S}_2\text{O}_3 \cdot 2 \text{ aq.}$ The crystals cannot therefore contain more than one molecule of water; and in view of their transparency and not changing in form on heating, it appears probable that they were anhydrous. The crystalline form was an obtuse rhombohedron. I have not been able to find any account of a salt containing less than five molecules of water.

A solution of sodium sulphate, which was distinctly supersaturated at 20°C. , readily dissolved a considerable quantity of the anhydrous salt when introduced as above, even at 15°C. Another solution deposited anhydrous salt at 30°C. , which redissolved on cooling to 20°C. ; while $\text{Na}_2\text{SO}_4 \cdot 7 \text{ aq.}$ crystallized out on cooling a few degrees. These crystals, however, quickly dissolved when the bottle was warmed in the hand. The most remarkable feature of these experiments was that in no instance was any caking of the anhydrous salt observed, as would have been the case had it combined with the water to form a hydrate before dissolving; it is, of course, evident that no trace of the normal hydrate was in any case produced, for this would have determined the crystallization of the supersaturated solution.

The only experiments in any way resembling those above described that I have been able to find an account of, are those of Coppet and Thompson.

Coppet* describes the manufacture of supersaturated solutions of sodium sulphate and other salts by the addition of anhydrous or partially dehydrated salt to water. He found that, in order to succeed, it was necessary to exclude unfiltered air and to add the salt in exceedingly small quantities at a time. With sodium sulphate he found that he could thus obtain a solution containing nearly five times as much anhydrous salt as is contained in a solution prepared from the decahydrated salt at the same temperature. In this case the addition of the anhydrous salt must have been so slow, that when the *ordinary* saturated solution was produced no decahydrated salt remained undissolved; or it is quite possible that no decahydrated salt is formed by the addition of anhydrous salt to any but very dilute solutions.

* *Comptes Rendus*, 1871, vol. lxxiii. p. 1324.

Again, J. S. Thompson*, in experimenting with potash-alum, found that a supersaturated solution, prepared by saturating water at 90° C. with the ordinary hydrate, deposited no crystals on cooling to the ordinary temperature; but that a solution saturated at 95° C. deposited small clear crystals. On breaking the flask in ice-cold water and washing the crystals, he found on analysis that they were ordinary alum with 24 molecules of water. This was of course to be expected as the natural result of his mode of experiment, for the anhydrous salt or a lower hydrate would at once take up water to form the normal hydrate; while it is equally evident that the crystals could not have been the normal hydrate, or they would have caused crystallization of the supersaturated solution in which they were formed.

There is therefore, it appears to me, little doubt that the so-called supersaturated solutions furnish a further proof of my contention that a salt exists in solution not as a hydrate definite or indefinite, but in the anhydrous state. This may appear a curious statement, but I fail to see what other explanation can be given of the water of crystallization possessing the same volume as the solvent water, as has been proved by the experiments by Ostwald and myself on the molecular volumes of solutions of nearly two hundred salts of various metals. This will not appear so strange when it is remembered that it is already pretty generally admitted that double salts as a rule exist only in the solid state, combination taking place only at the moment of crystallization from solution. I have already (*loc. cit.*) pointed out the fallacy involved in the argument from the heat of hydration; and the only difficulty that remains is the colour of certain salts in solutions of various degrees of concentration, or in the solid state. Thus anhydrous copper sulphate or the monohydrated salt is colourless, while the pentahydrate and the solution is blue. Again, cobalt chloride is red when hydrated and solid, blue when dehydrated; on the other hand, a strong solution of this salt is red when cold, blue when hot. This has always been regarded as a conclusive proof of the existence of the hydrated salt in at least dilute and cold solutions. But it is more than probable that the whole secret of the above colour-changes lies in the difference in the aggregations of molecules and not in the amount of hydration of the salt. We do not in the least know what multiple of the usual formula for a molecule forms the individual in the solid state or in solution; and it would be by no means a forced explanation of the above colour-changes to attribute them to the

* Journ. Chem. Soc. 1882, vol. xli. p. 382.

simplification of these aggregations of molecules. It would be quite possible that the volume-changes attending this disgregation would escape detection by the methods I have employed; but the changes in volume produced by hydration or nonhydration are far too large not to be detected by a comparison of the molecular volumes of the solutions, especially when dealing with salts containing a large number of molecules of water of crystallization. I do not at present wish to lay much stress on this explanation; it is, it seems to me, a point which has by no means received the attention which is its due; but I am convinced that until this is conclusively settled one way or another, it is premature to bring forward colour-changes as the sole evidence in favour of the hydration of salts in solution—a conclusion directly negatived by the molecular volumes of the very salts on the colours of whose solutions the whole hypothesis is based.

Further, such colour-changes are well known to occur in cases where no dehydration or other decomposition can occur, and where the only explanation is to be found in an alteration of the molecular constitution. Numerous instances are given in a paper by Carnelley on the colour of chemical compounds as a function of their atomic weights*; but one or two may be given here with advantage. Thus, mercuric oxide and red lead are both red when cold, but darken when heated, ultimately becoming almost black though no decomposition has taken place, as is proved by their regaining their original colour on cooling. Again, the temperature of boiling water is sufficient to turn the scarlet cuprous mercuric iodide black, the scarlet colour returning when the salt is cold. The differences in colour of mercuric sulphide are also due to a difference in molecular constitution. But perhaps the most instructive instance of all is to be found in the effect of heat on the hexhydrated cobalt chloride itself: when the solid crystals are gently warmed they change from red to blue without any loss of water, they do not lose their transparency, and the colour-change is found to have extended to the very centre of the crystal.

The results of my experiments may be summarized as follows:—

(1) The individual in solution is not the same as that in the solid state when hydrated salts are considered.

(2) It is to this that supersaturation is due.

(3) A supersaturated solution is a solution of the anhydrous salt, which may or may not be saturated. So long as no disturbing cause operates to bring about combination of

* *Phil. Mag.* 1884, xviii. p. 130.

the salt and water a supersaturated solution differs in no respect from an ordinary solution.

(4) The state of unstable equilibrium existing in a supersaturated solution is analogous to that existing in water cooled below its freezing-point.

(5) It is probable that full hydration of a dehydrated salt previous to solution occurs only when the salt is dissolved in pure water or in a very dilute solution of itself.

LII. *On Binocular Glasses adjustable to Eyes having unequal Focal Lengths.* By Colonel MALCOLM, R.E., C.B.*

WHETHER many people are born with their eyes differing in focal length or not I cannot say; it is sufficient that I have arrived at a period of life when glasses are necessary to enable me to read with comfort, and also that I found a very highly treasured pair of binocular glasses, by Voigtländer, become yearly more difficult to use, and at last *useless*; but I found that I could see with equal distinctness with each eye, using only one eye at a time, on condition of a very slight alteration of focus.

Mr. Browning, optician, in the Strand, having tested my eyes for a pair of pince-nez, told me that, as regards the ordinary reading and writing distances at any rate, my eyes were not a pair, and that after a certain age few people's were who had used their eyes much.

Accepting the fact, I set to work to make my binocular glasses once more useful—in this way.

One tube is left untouched; the eyepiece of the other is so arranged that it can be moved through a small range in and out, with reference to the eyepiece of the untouched tube, by turning round a milled ring. An index arrangement is provided.

The unaltered tube is used with one eye and brought to the most perfect focus possible in the ordinary way; then the other tube is used with the other eye, and by means of the adjustment its definition is made as perfect as may be, the ordinary adjustment not being interfered with. The two eyes are then used together; and the process of adjustment had better be gone over again, as certainly the two eyes do help each other.

The final position of the index-mark is noted; and that holds good for all ranges, as far as I have tried.

Having noted this, you may lend your glasses to your friend,

* Communicated by the Physical Society. The glasses were exhibited at the Meeting held on March 28, 1885.

who may alter them to his sight, and yet have them in perfect order for yourself by bringing the index to your own mark. The increase of power in the glasses, and of comfort to the user of them, has been a surprise to almost all who have tried the improved adjustment.

I do not enlarge upon the way in which Mr. Browning has carried out the details; you can see for yourselves that the improvement in no way disfigures the glasses, and as the expense is 12s. 6d. for glasses made to the common pattern, I think that the improvement is within the reach of most people who use binoculars.

LIII. *On the Self-Regulation of the Compound Dynamo.*

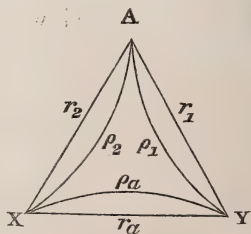
By A. W. RÜCKER, M.A., F.R.S.*

THE theory of the self-regulating power of the compound dynamo has not been treated in a satisfactory manner in any discussion of the subject with which I am acquainted. Some conclusions as to the proper method of constructing self-regulating dynamos have been drawn from equations in which the power of perfect self-regulation is assumed. As this perfection is known to be unattainable, no great weight can be given to arguments based on the assumption in question, and it will in the following paper be abandoned. A further inquiry will also be entered upon, viz. how far the conditions of good self-regulation are consistent with the other desiderata aimed at in the construction of a dynamo—such, for instance, as high efficiency.

The entire investigation will be based on Frölich's equation, which expresses the electromotive force in terms of the current and the constants of the machine, and will therefore not apply in cases where the prejudicial effect of the magnetization of the armature is apparent. The notation and diagrammatic representation of the machine are those which I have found useful for teaching purposes.

Let the points A, X, Y be joined by three straight and three curved lines.

Let the side XY represent the armature of the machine. The points A, X, and Y may be called its terminals. The curved lines represent the magnetizing coils of the inducing magnets; the straight lines AX and AY represent external resistances.



* Communicated by the Physical Society: received March 24, read April 25, 1885.

Let the values of the resistances be as indicated in the figure. Let the total electromotive force be E , that between Y and A e_1 , and that between A and X e_2 . The currents in r_1 and ρ_1 may be represented by c_1 and γ_1 , and a similar notation used for the other conductors.

Finally, let the whole resistance be R , let the resistances of the multiple arcs between $Y A$ and $A X$ be R'_1 and R'_2 respectively, and let c' be the total current in each of these multiple arcs.

The current is to be conceived as flowing out of the armature at Y , passing on to the three conductors ρ_a , ρ_1 , and r_1 , and returning at X .

All the existing forms of dynamo can be represented by leaving out some of the conductors shown in the above symmetrical figure. Their properties are perhaps more easily recognized by regarding them as particular cases of a more highly generalized machine. The notation used has also another advantage. The shunt in the Compound Dynamo may be used in one of two ways—viz. as a shunt upon the armature alone, or as a shunt upon the armature and series-coils. It is usual to represent its resistance by a symbol which indicates only that it is a shunt, without showing in which of these ways it is used. If, however, it is agreed that r_2 shall always represent the external resistance, r_1 being in any practical case infinite, then ρ_1 always represents the series-coils, and ρ_a or ρ_2 the shunt-coil according as it is applied in the first or second of the methods above referred to.

If we adopt Frölich's equation we may write

$$E = \frac{nM(s_0\gamma_0 + s_a\gamma_a + s_1\gamma_1 + s_2\gamma_2)}{1 + \sigma(s_0\gamma_0 + s_a\gamma_a + s_1\gamma_1 + s_2\gamma_2)};$$

where n represents the number of revolutions;

M is a constant depending on the machine;

s_a , s_1 , and s_2 are the number of turns made by the magnetizing spirals ρ_a , ρ_1 , and ρ_2 ;

and σ is a small constant depending on the gradual weakening of the magnetizing effects of the currents as the iron approaches saturation.

The product $s_0\gamma_0$ represents the number of ampere-turns by which the initial magnetic field is, or may be supposed to be, produced. If γ_0 is an independent current, this quantity may be large. If it is only a fictitious current which is regarded as the cause of the permanent magnetism, $s_0\gamma_0$ is small. If the product of $s_0\gamma_0$ and of σ may be neglected, and if $M s_0\gamma_0 = k$, we may write

$$E = nk + \frac{nM(s_a\gamma_a + s_1\gamma_1 + s_2\gamma_2)}{1 + \sigma(s_a\gamma_a + s_1\gamma_1 + s_2\gamma_2)};$$

We have at once

$$\frac{\gamma_a}{\rho_a} = \frac{c'}{R'_1 + R'_2} = \frac{c_a}{\rho_a + \frac{1}{R'_1 + R'_2}},$$

$$\gamma_1 \rho_1 = c' R'_1 \text{ and } \gamma_2 \rho_2 = c' R'_2.$$

Hence

$$E - nk = \frac{nM \left\{ s_a \frac{R'_1 + R'_2}{\rho_a} + s_1 \frac{R'_1}{\rho_1} + s_2 \frac{R'_2}{\rho_2} \right\} c'}{1 + \sigma \left\{ s_a \frac{R'_1 + R'_2}{\rho_a} + s_1 \frac{R'_1}{\rho_1} + s_2 \frac{R'_2}{\rho_2} \right\} c'}.$$

But

$$c' = \frac{\rho_a c_a}{R'_1 + R'_2 + \rho_a} = \frac{\rho_a}{R'_1 + R'_2 + \rho_a} \times \frac{E}{R}.$$

Hence, if we write

$$\left\{ s_a \frac{R'_1 + R'_2}{\rho_a} + s_1 \frac{R'_1}{\rho_1} + s_2 \frac{R'_2}{\rho_2} \right\} \frac{\rho_a}{R'_1 + R'_2 + \rho_a} = S,$$

we get

$$E - nk = \frac{nMSE}{R} \left/ \left(1 + \frac{\sigma SE}{R} \right) \right.,$$

or

$$(E - nk) \left(E + \frac{R}{\sigma S} \right) = \frac{nM}{\sigma} E.$$

If the machine has no external excitement k may be neglected, unless the conditions under which it begins to work are under investigation. Hence the equation becomes

$$E = \{nM - R/S\} / \sigma. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If there is external excitement and the magnets have not reached the condition where the evidences of approaching saturation become important, terms in σ may be neglected, and we have

$$E = \frac{nkR/S}{R/S - nM}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

If k and σ be both neglected these equations lead to conclusions of no practical value; for if $nM = R/S$, both (1) and (2) make E indeterminate; and if $nM \neq R/S$, we get $E = \infty$ from (1) and $E = 0$ from (2).

These results prove that no true approximation to the behaviour of the machine can be obtained by neglecting both the constants k and σ .

The above equations are exactly similar in form to those ordinarily given as applying to series and shunt machines;

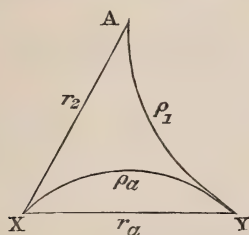
though, from the more general meaning assigned to the constants, they are in reality of a more general character.

Let us now apply them to the compound dynamo. Of the two forms of this machine, which, following the example of Prof. S. Thompson, we may call the Short-Shunt and the Long-Shunt respectively, we will take the former first.

Short-Shunt Compound Dynamo.

In this case $r_1 = \rho_2 = \infty$.

The conductors ρ_1 and ρ_a represent the series- and shunt-coils, and r_2 represents the external resistance.



Hence

$$R'_1 = \rho_1, \quad R'_2 = r_2,$$

$$R = r_a + \frac{\rho_a(r_2 + \rho_1)}{r_2 + \rho_1 + \rho_a} = \frac{(r_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a}{r_2 + \rho_1 + \rho_a},$$

$$S = \left\{ s_a \frac{r_2 + \rho_1}{\rho_a} + s_1 \right\} \frac{\rho_a}{r_2 + \rho_1 + \rho_a}.$$

Neglecting k , we get

$$E = \frac{1}{\sigma} \left\{ nM - \frac{(r_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a}{s_a(r_2 + \rho_1) + s_1 \rho_a} \right\}.$$

Again, since

$$\frac{e_2}{c_2 r_2} = \frac{E}{c_a R} \quad \text{and} \quad \frac{c_2}{\frac{1}{r_2 + \rho_1}} = \frac{\gamma_a}{\frac{1}{\rho_a}} = \frac{c_a}{\frac{r_2 + \rho_1 + \rho_a}{(r_2 + \rho_1) \rho_a}},$$

$$\begin{aligned} E &= e_2 \frac{r_2 + \rho_1 + \rho_a}{\rho_a} \times \frac{R}{r_2} \\ &= e_2 \frac{(r_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a}{r_2 \rho_a}. \end{aligned}$$

Whence

$$\begin{aligned} e_2 &= \frac{1}{\sigma} \left\{ \frac{nM r_2 \rho_a}{r_2(r_a + \rho_a) + \rho_1(r_a + \rho_a) + r_a \rho_a} \right. \\ &\quad \left. - \frac{r_2 \rho_a}{r_2 s_a + (s_a \rho_1 + s_1 \rho_a)} \right\}, \end{aligned}$$

which may be written

$$e_2 = \frac{P_1}{A_1 + x_1} - \frac{Q_1}{B_1 + x_1},$$

if

$$P_1 = \frac{nM\rho_a}{\sigma \{ \rho_1(r_a + \rho_a) + r_a\rho_a \}}, \quad A_1 = \frac{r_a + \rho_a}{\rho_1(r_a + \rho_a) + r_a\rho_a}, \quad (3)$$

$$Q_1 = \frac{\rho_a}{\sigma(s_a\rho_1 + s_1\rho_a)}, \quad B_1 = \frac{s_a}{s_a\rho_1 + s_1\rho_a}, \quad x_1 = \frac{1}{r_2}.$$

In like manner, since $c_2 = e_2/r_2$,

$$c_2 = \frac{1}{\sigma} \left\{ \frac{nM\rho_a}{r_2(r_a + \rho_a) + \rho_1(r_a + \rho_a) + r_a\rho_a} - \frac{\rho_a}{r_2s_a + (s_a\rho_1 + s_1\rho_a)} \right\},$$

which may be written

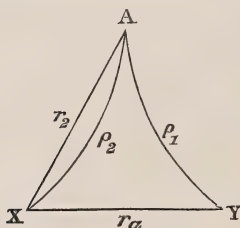
$$c_2 = \frac{P_2}{A_2 + x_2} - \frac{Q_2}{B_2 + x_2},$$

if

$$P_2 = \frac{nM\rho_a}{\sigma(r_a + \rho_a)}, \quad A_2 = \frac{\rho_1(r_a + \rho_a) + r_a\rho_a}{r_a + \rho_a}, \quad (4)$$

$$Q_2 = \frac{\rho_a}{\sigma s_a}, \quad B_2 = \frac{s_a\rho_1 + s_1\rho_a}{s_a}, \quad x_2 = r_2.$$

Long-Shunt Compound Dynamo.



In this case

$$r_1 = \rho_a = \infty.$$

The conductors ρ_1 and ρ_2 represent the series- and shunt-coils, and r_2 represents the external resistance.

Hence

$$R'_1 = \rho_1, \quad R'_2 = r_2\rho_2 / (r_2 + \rho_2),$$

$$R = r_a + \rho_1 + \frac{r_2\rho_2}{r_2 + \rho_2} = \frac{r_2(r_a + \rho_1 + \rho_2) + \rho_2(r_a + \rho_1)}{r_2 + \rho_2},$$

$$S = s_1 + s_2 \frac{r_2}{r_2 + \rho_2} = \frac{r_2(s_1 + s_2) + s_1\rho_2}{r_2 + \rho_2};$$

whence we get

$$E = \frac{1}{\sigma} \left\{ nM - \frac{r_2(r_a + \rho_1 + \rho_2) + \rho_2(r_a + \rho_1)}{r_2(s_1 + s_2) + s_1\rho_2} \right\}.$$

Also, since

$$\frac{e_2}{\frac{r_2\rho_2}{r_2 + \rho_2}} = \frac{E}{R},$$

$$e_2 = \frac{1}{\sigma} \left\{ \frac{nMr_2\rho_2}{r_2(r_a + \rho_1 + \rho_2) + \rho_2(r_a + \rho_1)} - \frac{r_2\rho_2}{r_2(s_1 + s_2) + s_1\rho_2} \right\};$$

which may be written

$$e_2 = \frac{P_3}{A_3 + x_3} - \frac{Q_3}{B_3 + x_3},$$

if

$$P_3 = \frac{nM}{\sigma(r_a + \rho_1)}, \quad A_3 = \frac{r_a + \rho_1 + \rho_2}{\rho_2(r_a + \rho_1)}, \quad . \quad . \quad (5)$$

$$Q_3 = \frac{1}{\sigma s_1}, \quad B_3 = \frac{s_1 + s_2}{s_1\rho_2}, \quad x_3 = \frac{1}{r_2}.$$

And, as before,

$$e_2 = \frac{P_4}{A_4 + x_4} - \frac{Q_4}{B_4 + x_4},$$

if

$$P_4 = \frac{nM\rho_2}{\sigma(r_a + \rho_1 + \rho_2)}, \quad A_4 = \frac{\rho_2(r_a + \rho_1)}{r_a + \rho_1 + \rho_2}, \quad . \quad . \quad (6)$$

$$Q_4 = \frac{\rho_2}{\sigma(s_1 + s_2)}, \quad B_4 = \frac{s_1\rho_2}{s_1 + s_2}, \quad x_4 = r_2.$$

We thus see that if the current or difference of potential which it is required to maintain as constant as possible be represented by the general symbol ϕ , then, in the case of either machine,

$$\phi = P / (A + x) - Q / (B + x);$$

where A, B, P, and Q do not depend upon the variable resistance, and where x is either the resistance or the conductivity of the variable conductor.

All problems on self-regulation in the case of either machine may therefore be solved by means of this formula, and the application of the results is made by giving to the constants the values proper to the machine, and to the meaning of the symbol ϕ in the particular case under consideration.

It will be observed that the constant A is always either the resistance, or the reciprocal of the resistance offered by the machine to a current produced by a battery placed in the

external circuit—*i. e.* it is either the resistance or the conductivity of the machine. If that resistance be represented by R_m , $A = R_m$ if ϕ is the external current, and $A = 1 / R_m$ if it is the external electromotive force.

If μ and m are respectively the largest and smallest values of x between which self-regulation is aimed at, then $\mu - m$ may be called the *range* of x . There will in general be some particular value of x which will be most frequently employed. This may be called the *usual value* of x , and may be denoted by ξ . As it is sometimes necessary to distinguish between the cases in which x is a resistance and a conductivity, the value of r_2 which corresponds to ξ will be indicated by \bar{r}_2 .

Conditions of Efficiency.

Although a self-regulating machine is primarily intended to supply the same current or electromotive force whatever (within certain limits) the resistance of the external circuit may be, it is nevertheless desirable that it should be as efficient as possible. It is well known that the efficiency is a maximum for a given value of the external resistance; and it is evident that the machine should not only have a high efficiency, but that the maximum should, if possible, be attained under the ordinary conditions of working.

1. Short-Shunt Machine.

Let η be the electrical efficiency. Then

$$\begin{aligned}\eta &= \frac{e_2^2}{r_2} \bigg/ \frac{E^2}{R} = \frac{\rho_a^2}{(r_2 + \rho_1 + \rho_a)^2} \times \frac{r_2}{R} \\ &= \frac{\rho_a^2}{r_2 + \rho_1 + \rho_a} \times \frac{r_2}{(r_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a}.\end{aligned}$$

Taking the logarithms of both sides and differentiating with respect to r_2 , we get

$$\frac{1}{\eta} \frac{d\eta}{dr_2} = \frac{1}{r_2} - \frac{1}{r_2 + \rho_1 + \rho_a} - \frac{r_a + \rho_a}{(r_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a},$$

which vanishes if

$$r_2^2(r_a + \rho_a) = (\rho_1 + \rho_a) \{ \rho_1(r_a + \rho_a) + r_a \rho_a \}. \quad (7)$$

Let r'_2 be the positive root of this equation. Hence, since

$$(r'_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a = (r'_2 + \rho_1) \rho_a + r_a \{ r'_2 + \rho_1 + \rho_a \},$$

and from (7),

$$r_a \{ r'^2_2 - (\rho_1 + \rho_a)^2 \} = \rho_a \{ \rho_1(\rho_1 + \rho_a) - r'^2_2 \},$$

or

$$\text{we have } r_a \{ r'_2 + \rho_1 + \rho_a \} = \rho_a \frac{\rho_1(\rho_1 + \rho_a) - r'^2_2}{r'_2 - (\rho_1 + \rho_a)},$$

$$\begin{aligned} & (r'_2 + \rho_1)(r_a + \rho_a) + r_a \rho_a \\ &= \rho_a \left\{ r'_2 + \rho_1 + \frac{\rho_1(\rho_1 + \rho_a) - r'^2_2}{r'_2 - (\rho_1 + \rho_a)} \right\} \\ &= - \frac{r'^2_2 \rho_a^2}{r'_2 - (\rho_1 + \rho_a)}. \end{aligned}$$

Substituting this in the expression for η , we get

$$\eta = \frac{\rho_1 + \rho_a - r'_2}{\rho_1 + \rho_a + r'_2}, \text{ or } \rho_1 + \rho_a = r'_2(1 + \eta)/(1 - \eta).$$

Hence, from (7),

$$\frac{r_a + \rho_a}{\rho_1(r_a + \rho_a) + r_a \rho_a} = \frac{1 + \eta}{1 - \eta} \times \frac{1}{r'_2};$$

and from (3) and (4),

$$A_1 = \frac{1 + \eta}{1 - \eta} \xi \text{ and } A_2 = \frac{1 - \eta}{1 + \eta} \xi, \quad . \quad . \quad . \quad (8)$$

if we suppose r'_2 to be $= \bar{r}_2$, *i. e.* if the machine is so designed that the usual value of the external resistance is that which gives the maximum efficiency.

2. Long-Shunt Machine.

In this case

$$\begin{aligned} \eta &= \frac{e^2_2}{r^2_2} \bigg/ \frac{E^2}{R} = \frac{\rho^2_2}{(r_2 + \rho_2)^2} \times \frac{r_2}{R} \\ &= \frac{\rho^2_2}{r_2 + \rho_2} \times \frac{r_2}{r_2(r_a + \rho_1 + \rho_2) + \rho_2(r_a + \rho_1)}; \end{aligned}$$

and, as before, $d\eta / dr_2 = 0$ if

$$\begin{aligned} \frac{1}{r'_2} - \frac{1}{r'_2 + \rho_2} - \frac{r_a + \rho_1 + \rho_2}{r'_2(r_a + \rho_1 + \rho_2) + \rho_2(r_a + \rho_1)} &= 0; \\ \therefore r'^2_2(r_a + \rho_1 + \rho_2) &= \rho^2_2(r_a + \rho_1), \quad . \quad . \quad . \quad (9) \end{aligned}$$

or

$$r_a(\rho^2_2 - r'^2_2) = r'^2_2(\rho_1 + \rho_2) - \rho_1 \rho^2_2.$$

Hence

$$\begin{aligned} & r_a(r'_2 + \rho_2) + r'_2(\rho_1 + \rho_2) + \rho_1 \rho_2 \\ &= \frac{r'^2_2(\rho_1 + \rho_2) - \rho_1 \rho^2_2}{\rho_2 - r'_2} + r'_2(\rho_1 + \rho_2) + \rho_1 \rho_2 \\ &= \frac{r'^2_2 \rho^2_2}{\rho_2 - r'_2}. \end{aligned}$$

Substituting in the expression for η , we obtain

$$\eta = \frac{\rho_2 - r'_2}{\rho_2 + r'_2}, \text{ or } \rho_2 = r'_2(1 + \eta) / (1 - \eta).$$

Hence, from (5), (6), and (9), if, as before, $r'_2 = \bar{r}_2$,

$$\left. \begin{aligned} A_3 &= \frac{1 + \eta}{1 - \eta} \times \frac{1}{\bar{r}_2} = \frac{1 + \eta}{1 - \eta} \xi, \\ A_4 &= \frac{1 - \eta}{1 + \eta} \times \bar{r}_2 = \frac{1 - \eta}{1 + \eta} \xi. \end{aligned} \right\} \dots \dots (10)$$

The above values for A will be required hereafter; but if we replace A by its value in terms of R_m , equations (8) and (10) may all be summed up in the formula

$$(1 - \eta) / (1 + \eta) = R_m / r'_2,$$

or

$$\eta = (r'_2 - R_m) / (r'_2 + R_m).$$

In the case of the Short-Shunt Machine,

$$r'^2_2 = (\rho_1 + \rho_a) R_m \text{ from equation (7).}$$

$$\therefore \eta = (\sqrt{\rho_1 + \rho_a} - \sqrt{R_m}) / (\sqrt{\rho_1 + \rho_a} + \sqrt{R_m}).$$

In the case of the Long-Shunt Machine,

$$r'^2_2 = \rho_2 R_m, \text{ from equation (9).}$$

$$\therefore \eta = (\sqrt{\rho_2} - \sqrt{R_m}) / (\sqrt{\rho_2} + \sqrt{R_m}).$$

These formulæ may be put in another form, which is useful as it is very easy to remember. In either kind of compound machine, and in the ordinary shunt dynamo, if the resistance of the armature is infinite, it is possible for a current generated in the external circuit to pass from one extremity of the external circuit to the other, *i. e.* from one terminal to the other, exclusively through wires which form parts of the magnetizing spirals. In the case of the Short-Shunt Machine this path includes both spirals; in the ordinary Shunt or in the Long-Shunt Compound Machine it includes one spiral only. If we call the resistance of this path Σ , then in all three cases the maximum efficiency is given by the equation

$$\eta = (\sqrt{\Sigma} - \sqrt{R_m}) / (\sqrt{\Sigma} + \sqrt{R_m});$$

and the value of the external resistance for which the maximum efficiency is attained by

$$r'^2_2 = \Sigma R_m.$$

In cases where the resistance of the shunt is large, approximate expressions may be deduced from the above by substituting

tuting the resistance of the shunt for Σ , and the sum of the resistances of the armature and series-coil for R_m .

If the maximum efficiency is attained when r_2 has its usual value, we may put the last expression in a form which will be useful hereafter. Let Y be a quantity which has with regard to Σ the same meaning as x with regard to r_2 ; *i. e.* let it be $=\Sigma$ or $1/\Sigma$ according as ϕ represents the external current or external electromotive force. Then

$$\eta = (\sqrt{Y} \sim \sqrt{A}) / (\sqrt{Y} + \sqrt{A});$$

and if the maximum efficiency is attained for the usual value of x ,

$$\xi^2 = YA.$$

Conditions of Maximum Power.

Since the power of a dynamo is expressed by either of the formulæ e_2^2/r_2 or $c_2^2 r_2$, it is in all cases given by $\phi^2 x$.

This has a critical value if

$$2x \frac{d\phi}{dx} + \phi = 0;$$

i. e. if

$$2x \left\{ -\frac{P}{(A+x)^2} + \frac{Q}{(B+x)^2} \right\} + \frac{P}{A+x} - \frac{Q}{B+x} = 0;$$

i. e. if

$$\frac{P(A-x)}{(A+x)^2} + \frac{Q(B-x)}{(B+x)^2} = 0.$$

Conditions of Self-Regulation.

Since

$$\phi = \frac{P}{A+x} - \frac{Q}{B+x},$$

$$\therefore \frac{d\phi}{dx} = -\frac{P}{(A+x)^2} + \frac{Q}{(B+x)^2}.$$

Hence ϕ has two critical values corresponding to the values of x given by the equation

$$x(\sqrt{P} \mp \sqrt{Q}) = -B\sqrt{P} \pm A\sqrt{Q}.$$

If we take the lower signs, x is necessarily negative unless B is negative; which can only be the case if some of the inducing spirals are wound in the negative direction, *i. e.* so that they reduce the strength of the magnetic field. If B is negative, it is evident that ϕ is infinite for a positive value of x . This unintelligible result is explained by reference to the

general equation in which both k and σ are included, viz.

$$(E - nk)(E + R / \sigma S) = nME / \sigma.$$

For if some of the quantities s_a , s_1 , or s_2 are negative, and if the value of r_2 is such that $S=0$, then $E=nk$. The approximate expression which is being discussed fails therefore in this case, but it applies to cases in which one of the spirals is wound in the negative direction but in which B remains positive. Reference to the expressions for B shows that this will be the case in the Short-Shunt Machine if the series-coils are negative and if the number of turns is $< s_a \rho_1 / \rho_a$, and in the Long-Shunt Machine if the shunt-coils are negative and if $s_2 < s_1$. In other cases Q is negative also.

Confining our attention therefore to the case in which B is positive, let X be the critical value of x , and Φ the corresponding value of ϕ .

Then

$$X = (A\sqrt{Q} - B\sqrt{P}) / (\sqrt{P} - \sqrt{Q}), \quad \dots (11)$$

$$\Phi = (\sqrt{P} - \sqrt{Q})^2 / (A - B). \quad \dots (12)$$

Also

$$\frac{d^2\phi}{dx^2} = \frac{2P}{(A+x)^3} - \frac{2Q}{(B+x)^3};$$

whence, at the critical point,

$$\frac{d^2\phi}{dx^2} = -2(\sqrt{P} - \sqrt{Q})^4 / (A - B)^3 \sqrt{PQ}. \quad \dots (13)$$

Hence if $A > B$, Φ is positive and is a maximum; and if $A < B$, Φ is negative and is a minimum.

The value of X given by (11) is not necessarily positive, and hence we must consider a number of cases which differ from each other in the relative magnitudes of A , B , P , and Q .

In distinguishing between them, it is convenient to remember that

$$\phi = \frac{x(P - Q) + BP - AQ}{(A + x)(B + x)}.$$

It follows from this expression and from (11) that

(I.) If $P/Q < A/B < 1$,

ϕ is negative for all positive values of x .

(II.) If $A/B < P/Q < 1$,

ϕ is positive for values of $x < (BP - AQ) / (Q - P)$.

(III.) If $A/B < 1 < \sqrt{P}/\sqrt{Q}$, and therefore $< P/Q$,

ϕ is positive for all positive values of x .

In both these last cases ϕ has no maximum value; for in (II.) X is positive but $> (BP - AQ) / (Q - P)$, while in (III.) it is negative.

Next, taking cases in which $A - B$ is positive, we see that

(IV.) If $A/B > 1 > P/Q$,

ϕ is negative for all positive values of x .

(V.) If $A/B > P/Q > 1$,

ϕ is positive for values of $x > (AQ - BP) / (P - Q)$.

(VI.) If $P/Q > A/B > \sqrt{P} / \sqrt{Q} > 1$,

ϕ is positive for all positive values of x .

In cases (V.) and (VI.) ϕ has a maximum value corresponding to a positive value of x .

(VII.) If $\sqrt{P} / \sqrt{Q} > A/B > 1$,

ϕ is positive for all positive values of x , and X is negative, so that there is no maximum value of ϕ .

The physical meaning of these different conditions may be best understood by considering the inequalities

$$P/Q > \text{ or } < 1 \text{ and } P/Q > \text{ or } < A/B.$$

If ϕ represents the external electromotive force, the first of these becomes, in the case of the Short-Shunt and Long-Shunt Machines respectively,

$$nM(s_a\rho_1 + s_1\rho_a) > \text{ or } < \rho_1r_a + \rho_1\rho_a + r_a\rho_a, \quad . \quad . \quad (14)$$

and

$$nMs_1 > \text{ or } < r_a + \rho_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

But, by equation (1), the condition that the velocity is greater or less than the critical speed is

$$nM > \text{ or } < R/S;$$

and if, in the case of the Short-Shunt Machine, we put $r_2 = 0$, this becomes for the values of R and S given above,

$$nM > \text{ or } < \frac{\rho_1r_a + \rho_1\rho_a + r_a\rho_a}{s_a\rho_1 + s_1\rho_a},$$

which is identical with (14).

Similarly, in the case of the Long-Shunt Machine, we get for the same condition,

$$nM > \text{ or } < \frac{r_a + \rho_1}{s_1},$$

which is identical with (15).

Hence, in these cases,

$$P/Q > \text{ or } < 1$$

according as the speed is greater or less than the critical speed for the resistance of the circuit when the external resistance is zero.

In like manner, it can be shown that if ϕ represents the external current, the expression $P/Q >$ or < 1 according as the speed is greater or less than the critical speed when the external resistance is infinite.

Inasmuch, however, as $x=1/r_2$ when ϕ is the external electromotive force, and $x=r_2$ when ϕ is the external current, these results may be briefly expressed by saying that P/Q is $>$ or < 1 , according as the speed is greater or less than the critical speed when x is infinite.

Similarly it may be proved that

$P/Q >$ or $< A/B$ according as the speed is greater or less than the critical speed when x vanishes.

On referring to the conditions laid down in the seven cases distinguished above in the light of these explanations, we see that they may be readily interpreted. The constant P is proportional to the speed, and we may conceive the ratio P/Q to be gradually increased by augmenting the velocity of revolution.

Thus when $A-B$ is positive, Case IV. is that in which the velocity is less than the critical velocities when $x=0$ or ∞ .

In Case V. the velocity is greater than the critical velocity when $x=\infty$, and less than that when $x=0$.

In Cases VI. and VII. it is greater than either of these critical velocities.

The condition that there shall be a maximum value of ϕ is

$$A/B > \sqrt{P} / \sqrt{Q} > 1.$$

As an example of how such general formulæ can be applied to a particular case, we deduce from this relation and from equations (3) that, in the case of the Short-Shunt Dynamo, the external electromotive force can only have a maximum value if

$$\frac{r_a + \rho_a}{\rho_1(r_a + \rho_a) + r_a \rho_a} \cdot \frac{s_a \rho_1 + s_1 \rho_a}{s_a} > \sqrt{\left(\frac{nM(s_a \rho_1 + s_1 \rho_a)}{\rho_1(r_a + \rho_a) + r_a \rho_a} \right)} > 1;$$

or if

$$\frac{\rho_1 + \frac{s_1}{s_a} \rho_a}{\rho_1 + \frac{r_a}{r_a + \rho_a} \cdot \rho_a} > \frac{nMs_a}{r_a + \rho_a} > \frac{\rho_1 + \frac{r_a}{r_a + \rho_a} \cdot \rho_a}{\rho_1 + \frac{s_1}{s_a} \rho_a};$$

whence it follows that we must have

$$\frac{s_1}{s_a} > \frac{r_a}{r_a + \rho_a}.$$

Similar deductions can be made in the other cases.

The fact that if P/Q be sufficiently large there is no maximum value of ϕ for any positive value of x , means that if the velocity is so great that the inducing magnets are saturated so that the total electromotive force is constant, any increase in the resistance of the external circuit must be accompanied by a decrease in the current which flows through it and an increase in the electromotive force at its extremities. In the former case $x=r_2$, and in the latter $x=1/r_2$, so that in both cases an increase in x produces a decrease in ϕ . This state of things is reached before complete saturation, viz. at the point when the prejudicial effect due to the weakening of the current produced by an increase in r_2 overbalances the advantage gained by the fact that a larger proportion of the whole current passes through the shunt. This point is reached when

$$\sqrt{P} / \sqrt{Q} = A / B.$$

We are now in a position to discuss the amount of change in ϕ produced by a given finite change in x , i. e. the self-regulating power of the dynamo.

The treatment of this question must be slightly varied, according as a maximum value of ϕ does or does not occur for a value of x intermediate to μ and m , the largest and smallest values of that quantity between which self-regulation is aimed at.

If it does not, we may write

$$\left. \begin{aligned} \frac{P}{A+m} - \frac{Q}{B+m} &= \phi_1, \\ \frac{P}{A+\mu} - \frac{Q}{B+\mu} &= \frac{\phi_1}{1+q}, \end{aligned} \right\} \dots \dots (16)$$

where ϕ_1 is the value of the electromotive force or current to be kept constant, and q is a quantity which will be smaller as the self-regulation is more perfect, and which will be positive or negative according as ϕ diminishes or increases as x increases.

If, on the other hand, X lies between μ and m , then the equations corresponding to (16) become

$$\left. \begin{aligned} \frac{P}{A+m} - \frac{Q}{B+m} &= \frac{\Phi}{1+p'}, \\ \frac{P}{A+\mu} - \frac{Q}{B+\mu} &= \frac{\Phi}{1+p''}, \end{aligned} \right\} \dots \dots (17)$$

where Φ is the maximum value of ϕ .

If the constants are so chosen that the values of ϕ corre-

sponding to μ and m are equal, then

$$\frac{P}{A+m} - \frac{Q}{B+m} = \frac{P}{A+\mu} - \frac{Q}{B+\mu} = \frac{\Phi}{1+p'}, \quad \dots (18)$$

where Φ is the maximum value of ϕ .

If, now, the following quantities are given, viz. :—

- (1) the value of ϕ which is to be attained (ϕ_1 or Φ);
- (2) the extreme values of x (μ and m);
- (3) the percentage variation in the value of ϕ which can be allowed ($100q$ or $100p$);

(4) the usual value of x (ξ), together with the condition that the maximum efficiency shall be attained for that value ;

(5) the value of the required maximum efficiency ;

then we have four equations, viz. *either* (16) or (17) and the two equations which connect η and ξ with A and Y (see p. 471) to be satisfied by the five constants A , Y , B , P , and Q . Of these, A and Y are absolutely determined by the conditions ; but any one of the three P , Q , or B may be given any convenient value if the maximum value of Φ does not lie between μ and m . If it does, we have the additional relation

$$\Phi = (\sqrt{P} - \sqrt{Q})^2 / (A - B);$$

and if the values of both p' and p'' are assigned, there are in all five equations by which the five constants are determined. When these five quantities are known, we may equate them to their appropriate values in terms of the resistances &c. of the various parts of the machine ; and we thus obtain five equations between eight quantities, viz. :— n , M , σ , s_1 , s_a (or s_2), r_a , ρ_1 , ρ_a (or ρ_2) ; in the selection of which, therefore, there is considerable range for choice.

Although the problem of the Self-Regulation of the Compound Dynamo is solved as far as the algebra is concerned, it is possible that the values of P , Q , &c. found for arbitrarily selected values of ϕ , μ , m , &c. may be negative, or such as it would be impossible to attain in practice.

The question as to what value of q it is physically possible to attain in any given case can only be answered if we have regard to the limitations to the values of A , B , P , and Q which apply to that case. If this be done, it is possible to obtain limits to q , that is to the perfection of self-regulation.

I shall add two examples of the use of equations (16) and (18) for this purpose.

Case III. $A < B$, $P > Q$.

As the values of ϕ diminish as x increases, the value

which corresponds to μ must be less than that which corresponds to m . Hence

$$\frac{P}{A+m} - \frac{Q}{B+m} = \phi_1,$$

$$\frac{P}{A+\mu} - \frac{Q}{B+\mu} = \frac{\phi_1}{1+q}.$$

Solving for P and Q , we get

$$P = \frac{\phi_1}{1+q} \cdot \frac{\mu-m-q(B+m)}{(A-B)(\mu-m)} (A+\mu)(A+m),$$

$$Q = \frac{\phi_1}{1+q} \cdot \frac{\mu-m-q(A+m)}{(A-B)(\mu-m)} (B+\mu)(B+m).$$

Now since $A-B$ is negative, we must, if P and Q are positive, have

$$q > (\mu-m)/(A+m),$$

and, *a fortiori*,

$$> (\mu-m)/(B+m).$$

Further, since $P > Q$,

$$\{q(B+m) - (\mu-m)\}(A+\mu)(A+m) >$$

$$\{q(A+m) - (\mu-m)\}(B+\mu)(B+m);$$

$$\therefore q(A+m)(B+m)(B-A) < (\mu-m)\{B^2 - A^2 + (\mu+m)(B-A)\};$$

$$\therefore q(A+m)(B+m) < (\mu-m)\{A+B+\mu+m\}.$$

Cases V. and VI.— $A/B > \sqrt{P}/\sqrt{Q} > 1$.

Taking next the case in which the values of ϕ corresponding to μ and m are equal, we deduce from (18)

$$P = \frac{\Phi}{1+p} \frac{(A+\mu)(A+m)}{A-B},$$

$$Q = \frac{\Phi}{1+p} \frac{(B+\mu)(B+m)}{A-B}.$$

Substituting these values in the equation

$$\Phi = (\sqrt{P} - \sqrt{Q})^2 / (A-B),$$

we get

$$(A-B)^2(1+p) = \{\sqrt{(A+\mu)(A+m)} - \sqrt{(B+\mu)(B+m)}\}^2;$$

which reduces to

$$\{(A-B)p + \mu - m\}^2 = 4p(A+m)(B+\mu).$$

Solving for B, we get

$$Bp = Ap + \mu + m + 2A \pm 2\sqrt{(A+\mu)(A+m)(1+p)}.$$

Hence, since $A > B$, Ap must be greater than the right-hand side of the equation. This inequality can only hold if we take the negative sign, and if

$$2\sqrt{(A+\mu)(A+m)(1+p)} > (A+\mu) + (A+m),$$

i. e. if

$$4p(A+\mu)(A+m) > (\mu-m)^2;$$

whence

$$p > \frac{(\mu-m)^2}{4(A+\mu)(A+m)}.$$

Also, since B is positive,

$$Ap + (A+\mu) + (A+m) > 2\sqrt{(A+\mu)(A+m)(1+p)};$$

$$\therefore 1+p - 2\sqrt{1+p}\sqrt{(A+\mu)(A+m)}/A \\ + (A+\mu)(A+m)/A^2 > \mu m / A^2;$$

$$\therefore \{\sqrt{1+p} - \sqrt{(A+\mu)(A+m)}/A\}^2 > \mu m / A^2.$$

We find, therefore, that in both these cases an inferior limit can be found to p or q , which depends only on μ , m , and A . If, therefore, the latter quantity is determined by considerations relative to the efficiency, we implicitly determine at the same time a limit to the perfection of the self-regulation.

The last case is probably important practically, and the expression obtained shows that the inferior limit to p varies, if A is large, nearly as the square of the range. It will be less as A is greater; and this statement holds good for all the other cases enumerated above, so that a large value of A is favourable to good self-regulation. On referring to equations (8) and (10), we see that for a given usual value of x a high maximum efficiency is favourable to a large value of A if ϕ is the external electromotive force, and to a small value of A if ϕ is the external current. Hence we conclude that it is more difficult to combine a high efficiency with an approximately constant external current than with an approximately constant external electromotive force.

Postscript, April 30, 1885.

Since the above paper was read before the Physical Society,

I have seen a solution of the question with which it deals, published by Dr. Frölich in the current number of the *Elektrotechnische Zeitschrift*, April 1885. I should like, therefore, to add a few words as to the relations between our investigations in respect both of time and result.

The method and chief conclusions given in my own paper, including the formulæ for ϕ , q , and the method of finding limits to the perfection of the regulation, were given to my Senior Class in Technical Electricity in February last. In March I had several engagements which made special calls upon my time, and partly on this account, partly because I wished to add a discussion of the case when Q or B is negative, I did not publish my results at once.

Towards the latter end of March I received my copy of the *Elektrotechnische Zeitschrift* for that month, which contained the first part of Dr. Frölich's paper. In that he gave the formulæ for the electromotive force, current, &c. in the cases of the series, shunt, and both forms of compound dynamo, in terms of the external and other resistances. The formulæ were not reduced to any simple form like the equation in ϕ given by me; and though the paper showed that Dr. Frölich was engaged on the question of self-regulation, it did not afford any indication of his method of attacking the problem. The next day after seeing this paper I sent my own investigation to the Secretary of the Physical Society. It was brought before the Council of the Society on March 28th; but as it was thought that the matter was of some interest, the reading of the paper was postponed till the first Meeting after the Easter vacation (April 25th), and I was asked to attend for the purpose of giving an account of it myself. In the interval I made some improvements in the statements as to the physical meanings of the inequalities which I described at the Meeting, and have incorporated these in the text. It is only since the paper was read that I have received the April number of the *Elektrotechnische Zeitschrift*, from which I learn that Dr. Frölich has "seit dem Abdrucke des ersten Theiles dieses Vortrages," and guided partly by theory, partly by experiment, arrived at a very satisfactory solution of the problem of obtaining a constant external electromotive force.

As far, therefore, as our work is common, I do not wish to question Dr. Frölich's undoubted right to claim prior publication. The above statement is, however, necessary to show that my results were obtained independently.

On comparing the two papers, it will be seen that I have throughout considered the problem with reference to the

efficiency as well as to the self-regulation, and have given the equations a more general form, in which they apply, when the symbols are properly interpreted, either to the external electromotive force or to the external current. Dr. Frölich has dealt chiefly with the case of a constant electromotive force, in which the conditions of high efficiency and good self-regulation are in accord, and both can be secured together. To obtain the best result, if a constant current is required, will need a careful balancing of opposing requirements.

As regards, however, the practical determination of another condition which shall render the problem determinate when a maximum value of ϕ does not occur between μ and m , Dr. Frölich has advanced the question a step beyond where I left it, and has added very greatly to the interest of his paper.

He has shown that when the external electromotive force is in question, and without any reference to the value of the external resistance for which the maximum efficiency occurs, excellent results may be obtained by taking (in the above notation) $A^2Q=B^2P$. I had previously shown the physical meaning of this condition; geometrically it amounts to choosing the arbitrary constant, so that $d\phi/dx=0$ when $x=0$. Since, in the case under discussion, $x=1/r_2$, Dr. Frölich's curves and mine are so related that the product of the abscissæ of corresponding ordinates is unity, and the tangent to my curve at the point where it meets the axis of ϕ becomes in Dr. Frölich's figures an asymptote.

As expressed by the curve between e_2 and r_2 (Dr. Frölich's), good self-regulation can only be obtained in this case for values of r_2 greater than that at which the curve may be considered to have become practically parallel to its asymptote. As expressed by the curve between e_2 and $1/r_2$ (my own), the corresponding condition is that μ shall be small enough for the curve to be at the corresponding point practically parallel to the tangent at the point for which $x=0$. Dr. Frölich has shown that these conditions are fulfilled in practice, and has thus added very materially to the importance of his paper.

It must, however, be noted that this solution has nothing absolute about it unless the efficiency is considered. If it is left out of account another constant (A) is indeterminate, and may be chosen so as to make q a minimum. For when $A^2Q=B^2P$,

$$q = \frac{\mu - m}{(A + m)(B + m)} \times \frac{AB(\mu + m) + \mu m(A + B)}{AB + \mu(A + B)}.$$

In the case under consideration we may put $m=0$, and thus

write

$$q = \mu^2 / \{AB + \mu(A + B)\};$$

which shows that, among the various solutions which satisfy the condition $A^2Q = B^2P$, that will give the best self-regulation for which A and B are largest.

The condition that A shall be large implies (since $A = 1/R_m$) that the resistance of the machine must be small, the necessity for which could of course be readily foreseen. The condition that B should be large leads to less obvious conclusions. Thus, in the case of the Long-Shunt Machine,

$$B_3 = (s_1 + s_2) / s_1 \rho_2;$$

and to make this as large as possible, we should have s_2/s_1 large and ρ_2 small. The first of these conditions is always, the second is never, fulfilled in practice. The reason of this is obvious, viz. that the efficiency would be reduced by diminishing the resistance of the shunt. The maximum efficiency is given by the formula

$$\eta = (\sqrt{\rho_2} - \sqrt{R_m}) / (\sqrt{\rho_2} + \sqrt{R_m}),$$

which diminishes with ρ_2 . It is interesting therefore to note that a high shunt-resistance is not in itself conducive to good self-regulation; and that, within the bounds of Dr. Frölich's condition, there are still opportunities for choice by which improvements in the efficiency and regulation may be effected. Probably in practice the last adjustments will be best made by some system of experiment like that described by Dr. Frölich. The theory given above will, however, enable much to be done by a few preliminary calculations.

Since I read the paper, Prof. Silvanus Thompson has called my attention to the question as to whether my formulæ indicate the advantage of back winding in the case when a constant external current is desired. Dr. Frölich has constructed a machine on this principle, to which he was led by theory. To discuss this question fully would require an investigation of the problem when B is negative; but it may be remarked that in a case such as that discussed above, when a large value of B is desirable, it may be increased in the case of the Long-Shunt Machine (B_4) by taking s_2 negative and $< s_1$.

LIV. *On the Identity of Energy: in connection with Mr. Poynting's Paper on the Transfer of Energy in an Electromagnetic Field; and on the two Fundamental Forms of Energy.* By OLIVER LODGE, D.Sc.*

IT is well known that Prof. Poynting has communicated to the Royal Society a most admirable and important paper, "On the Transfer of Energy in the Electromagnetic Field" †; a paper which cannot but exert a distinct influence on all future writings treating of electric currents.

In that paper he introduces the idea of continuity in the existence of energy—a natural though not a necessary consequence of its conservation; so that, whenever energy is transferred from one place to another at a distance, it is not to be regarded as destroyed at one place and recreated at another, but it is to be regarded as transferred, just as so much matter would have to be transferred; and accordingly we may seek for it in the intervening space, and may study the paths by which it travels.

This notion is, I say, an extension of the principle of the conservation of energy. The conservation of energy was satisfied by the *total quantity* remaining unaltered; there was no individuality about it: one form might die out, provided another form simultaneously appeared elsewhere in equal quantity. On the new plan we may label a bit of energy and trace its motion and change of form, just as we may ticket a piece of matter so as to identify it in other places under other conditions; and the route of the energy may be discussed with the same certainty that its existence was continuous as would be felt in discussing the route of some lost luggage which has turned up at a distant station in however battered and transformed a condition.

In this new form the doctrine of the conservation of energy is really much simpler and more satisfactory than in its old form; and the doctrine may be proved rigidly and instantaneously from two very simple premises, viz. Newton's law of motion on the one hand, and the denial of action at a distance on the other; as I endeavoured in this Magazine some time ago to show‡, and will now repeat.

I speak of Newton's *law* of motion because I believe it will be admitted that Newton's three laws of motion, in so far as

* Communicated by the Author.

† Poynting, Phil. Trans. ii. 1884, p. 343.

‡ Phil. Mag. January 1881, p. 36; and June 1881, p. 531. Also 'Elementary Mechanics' (Chambers), § 80.

they are more than definitions, are really three very important aspects of one law*. They may be regarded as (1) a definition of time, (2) a definition of force, (3) a statement of a law of Nature.

The law of Nature they embody is capable of various modes of expression, such as these (in brief):—

Change of Momentum = Impulse.

$$\text{Resultant force} = \frac{d(mv)}{dt}.$$

Action + Reaction = 0.

Force is always one component of a stress.

The last form is perhaps as convenient as any for our present purpose, and is our first lemma.

To deny action at a distance is easy; we have only to say, "If a stress exist between two bodies they must be in contact." This constitutes a second lemma.

We then only further require the definition of work and energy; for instance, these:—A body does work when it exerts force through a distance; the measure of work being $\int Fds$. Energy is that which a body loses when it does work; and it is to be measured as numerically equal to the work done. [The repetition with mere change of sign, about gain of energy when negative work is done by a body, or positive work done upon it, may be understood.]

Now at once follows, simply and rigorously, the law of the conservation of energy; and not only conservation, but conservation in the new form, viz. the *identification* of energy; thus: If A does work on B it exerts force on it through a certain distance; but (Newton's law) B exerts an equal opposite force, and (being in contact) through exactly the same distance; hence B does an equal opposite amount of work, or gains the energy which A loses. The stress between A and B is the means of transferring energy from A to B, directly motion takes place in the sense AB. And the energy cannot *jump* from A to B, it is transferred across their point of contact, and by hypothesis their "contact" is absolute: there is no intervening gap, microscopic, molecular, or otherwise. The energy may be watched at every instant. Its existence is continuous; it possesses identity.

It is no use objecting that two pieces of "matter" are never in contact—nobody said they were. If they are not, and it seems quite certain that they are not, then evidently one

* For argument in support of this view, see 'The Engineer,' 1885, March 20, April 24, May 15.

piece of "matter" cannot act immediately on another piece. A and B therefore are not two pieces of "matter" in the ordinary sense. A may be a molecule of matter, M may be the nearest molecule to it, and energy may be transferred from A to M, but not directly; A cannot act on M, cannot do work on it, because of the intervening gap. A can act on B, transferring its energy to B, B can act on C, C on D, and so on, handing on the energy to L, which is in contact with, and can act on, M, doing work on it and giving up to it the energy lost by A.

What B, C, D, L are, I do not presume to say; but of course one supposes them to be successive portions of the perfectly continuous space-filling medium *Æther*.

Relation between Potential and Kinetic Energy, from the contact point of view. Reason of the two forms; and Transformation into one another.

In the older and more hazy view of conservation of energy the idea of "potential energy" has always been felt to be a difficulty. It was easy enough to take account of it in the formulæ, but it was not easy or possible always to form a clear and consistent mental image of what was physically meant by it.

A stone is raised, it gains potential energy; but how does the stone "up" differ from the stone "down"? and how can an inert and quiet stone be said to possess energy? Well, then, the stone hasn't the energy but the earth has, or rather "the system of earth and stone possesses energy in virtue of its configuration." True, but foggy. The usual ideas and language current about potential energy are proper to notions of action at a distance. When universal contact action is admitted, the haze disappears*; the energy is seen to be possessed, not by stone or by earth or by both of them, but by the medium which surrounds both and presses them together; and the following statement may be made.

Energy has two fundamental forms because work has two factors, force and motion, *F, s*.

Work cannot be done except by a body exerting force *and* in motion. Force without motion is no good. Motion without force is no good. Either factor separately may be energy, but it is not work.

* It is by no means intended that the natures of gravitation, elasticity, cohesion, &c. become clear. What is meant is, that the *seat* of the energy is clearly recognized: the *reason* of the stress recognizable in the medium is a much higher and more difficult problem.

The two forms of energy correspond to the factors in the product work*.

"Potential" energy correspond to F.

"Kinetic" energy correspond to s.

But is this quite true and satisfactory? A strained bow is exerting force and possesses energy. A pillar supporting a roof is exerting force, but possesses no energy. What is the difference between the two cases? It is evident that something more is needed than mere force.

The difference of course is that the bow can recoil, it has a *range* or distance through which it will continue to exert a force: not the original force, but still some force. The pillar is exerting a great force but it has no recoil in it; if released it would at once cease to exert any force; consequently its energy is minute.

Thus, then, for a body to possess potential energy we must have two things—the exertion of a force, together with a guarantee that that force shall be exerted over a certain distance; *i. e.* a continuance of the force even after motion is permitted.

And this is quite analogous to what may be said of the other form of energy. "A body in motion possesses energy;" but is it so necessarily? Cannot a body in motion be conceived as possessing no energy? Suppose it stops the instant you give it work to do—the instant you make it exert force. It is evident you must have not merely motion, you must have a guarantee of *persistence* of motion, the body must possess inertia; the motion must continue over a certain range even against resistance. Hence we may exhibit the relation between the two forms thus:—

Kinetic energy corresponds to motion combined with inertia,
so that the motion shall continue even against some force;
and

Potential energy corresponds to force combined with
elasticity (or something like it), so that the force shall
continue even though motion be permitted.

Both forms of energy are *potential work*, but, *qua* energy, one is as real and actual as the other. Each has a factor missing, which if supplied, work will at once be done. Kinetic energy requires the Force factor to do work. Potential energy requires the Motion factor to do work.

An important thing is now evident moreover, a thing which I have never seen accepted, though it has been pre-

* Cf. Lodge on "Forms of Energy," Phil. Mag. October 1879, p. 281, and June 1881, p. 531. Also 'Elementary Mechanics' (Chambers), § 84.

vously pointed out*. The statement is in two parts:— (1) *Energy cannot be transferred without being transformed*; and (2) *it always transforms itself from Kinetic to Potential, or vice versâ.*

When A does work on B energy is transferred from A to B; and I say that if the energy which A lost was kinetic, then what B gains is potential; if, on the other hand, A loses potential, then B gains kinetic.

I may make a converse statement, viz. that *energy cannot be transformed without being transferred*; cannot take on a different form without being at the same time shifted to a different body. So that the common mode of treating a falling weight, saying that its energy gradually transforms itself from potential to kinetic but remains in the stone all the time, is, strictly speaking, nonsense. The fact is the stone never had any potential energy, no rigid body can have any; the gravitation medium had it however, and kept on transferring it to the stone all the time it was descending.

The above statement, that transformation of energy necessarily goes on from potential to kinetic or *vice versâ* at every act of transfer, almost proves itself. It follows from the following facts:—When a body possessing potential energy does work, its “range” necessarily diminishes, while the motion of the body on which the work is done increases. On the other hand, when a moving body does work its motion diminishes, and the body which resists the motion, since it yields over a certain distance, gains potential energy. For the first case think of a catapult, bow and arrow, or air-gun. For the second case think of a bullet fired against a spring and caught by it.

These examples are favourable and easy, but any others will serve equally well to illustrate the matter if regarded from the right point of view. Thus a bullet fired upwards gradually transfers its undissipated energy to the gravitation medium, transforming it at the same time into potential. As soon as the highest point is reached, the gravitation medium proceeds to re-transfer and transform it. A pendulum exhibits the alternation of energy from the kinetic to the potential form and the accompanying transfer from matter to medium, at every half-swing. Any vibrating body does the same; but in considering a strained spring we must remember that the energy resides not in the spring as a whole but in its elementary parts. The strain resides not even in the molecules themselves, perhaps, but in something between those molecules (for

* Phil. Mag. October 1879, p. 281, sect. 11, and June 1881, p. 532. And ‘Elementary Mechanics,’ § 85.

by hypothesis molecules are incapable of exerting *direct* force on one another, not being in contact). The medium is exerting the force, and will continue to exert it over a certain range, hence it possesses the potential energy; when released it will do work and transfer its energy to the steel, in the kinetic form. If the spring overshoots its mean position, the energy is re-transferred and transformed. A perfectly elastic bounding ball has all its energy transformed into potential, at the middle of every period of contact with the obstacle from which it rebounds.

One case possesses perhaps a little more difficulty, viz. the case of a bullet fired into dough—when the body exhibits no recoil: how can the energy of the bullet be said to be transformed into potential now? Only by remembering that heat-motion is a vibration of some kind, and that when a vibration is excited by a blow or by friction, *strain* is the effect first produced, and afterwards the recoil. Think, for instance, of exciting a tuning-fork or a string by bowing or striking it.

Alternations from kinetic to potential may be rapid in some cases, slow in others: no matter; all I have stated is that change of form is necessary and universal whenever energy is transferred, *i. e.* whenever any kind of activity is exhibited by any known kind of material existence.

University College, Liverpool,
May 16, 1885.

LV. *On the Paths of Electric Energy in Voltaic Circuits*
Appendix to Paper on the Seat of the Electromotive Forces in
the Voltaic Cell. By Prof. OLIVER LODGE*.

[Plates IV. & V.]

THE main conclusions to which I have been led with regard to the potentials of metals and the seat of E.M.F. may be very briefly stated thus:—

(1) A metal is not in general at the same potential as the air in contact with it; the difference of potential (or contact-force) between any given clean metal and air being calculable, at least approximately, from thermo-chemical data, though there is no known way of experimentally observing it.

(2) Putting two metals into contact equalizes their poten-

* Communicated by the Author. For the sake of completeness it may be convenient here to mention that a report of a discussion on Seat of E.M.F., at Montreal (Brit. Assoc., Sect. A), is published in the 'Electrical Review' for Nov. 22, 1884, and that a more elaborate discussion on the same subject, by the Society of Telegraph Engineers and Electricians, will be reported in their volume of 'Proceedings' next issued after the present date.

tial, their surfaces becoming charged with the electrostatic charges proper for effecting this equalization of potential.

(3) The air surrounding a pair of different metals in contact is therefore in a calculable condition of dielectric strain, especially near the junction of the two metals; in other words, there is in the air a slope of potential which is most rapid in the neighbourhood of the junction, and is there easily observed electroscopically.

(4) This slope of potential observed by Volta, measured by Kohlrausch &c. &c., near the junction of any two metals, has caused observers to imagine that the metals themselves differed in potential, and accordingly to postulate an E.M.F. or contact-force resident at the junction in order to maintain such difference of potential in a conductor.

(5) No such force exists at a metallic junction; because, if it did, the passage of a current across such a junction would produce violent reversible thermal effects, and none such are observed.

(6) This non-existent force has been imagined, and has been utilized to account for the greater part of the E.M.F. in a voltaic circuit; in fact, it has itself been considered to be the main E.M.F. in such a circuit; although it was manifestly unable to account for the energy of the current.

(7) The real seat of the E.M.F. in a voltaic or any other circuit must be where conversion of energy, from some other form (chemical, thermal, &c., &c.) into the form known as an electric current, occurs.

The object of the present Appendix or Supplement to my paper is to supply an accidental omission in the history of the subject, and more particularly to exhibit and illustrate the mode of regarding conducting circuits which I advocate, by diagrams of energy-paths, or *erg-odes* for those who like Greek barbarities.

The accidental omission from the history of the subject is a notice of Dr. Gore's communication to the Royal Society of November 1883*, in which he makes an attempt to bring liquids into a thermoelectric series.

The contact-force between a metal and a liquid is shown to vary with temperature; and the results indicate that this contact-force is a complex one, not simply depending on the chemical relations of the substances in circuit, *i. e.* on the cor-

* Gore, Proc. Roy. Soc. No. 233, vol. xxxvii. p. 251: "Some relations of Heat to Voltaic and Thermoelectric Action of Metals in Electrolytes." See also former papers in Phil. Mag. January 1857; Proc. Roy. Soc. 1878, p. 513, No. 188; 1879, No. 199, and 1880, No. 208.

rodibility of the metals, but involving some other more purely physical force also :—much as Bouty surmised from his experiments on the generation of heat at a metal-liquid junction when a current was driven across it.

A number of tables of the order of metals in different solutions of different strengths and at different temperatures are given, together with some numerical values.

It is possible that by an attentive study of Dr. Gore's work, and of other work in the same direction, a more complete account could be given of the exact relation existing between what may be called the thermal and the chemical contact-forces at metal-liquid junctions ; if indeed any valid distinction can actually be drawn between them.

At present, at any rate, I do not attempt this ; but proceed to the other matter spoken of, viz. the illustration of my views on the voltaic circuit by a diagram of its energy-paths.

Prof. Poynting has taught us, in a paper of the very greatest interest and power*, that modern views of *vis a tergo*, as opposed to action at a distance, lead necessarily to the idea of continuity in the existence of electric, as well as of all other, energy, and hence to a study of the paths along which it moves from one part of the field to another. He has shown that these paths are the intersections of the magnetic and the electrostatic equipotential surfaces, and that the rate of flow of energy is proportional to the product of the electrostatic and electromagnetic intensities at every point.

This view of the electromagnetic field, though no doubt implicitly involved in Maxwell's equations, yet urgently needed to be dragged forth and exhibited ; and it is difficult to imagine how the developing process could have been done better than it is done in Prof. Poynting's memoir.

The special cases by which his memoir is illustrated are not, however, in exact accordance with the views of the electric circuit which I am advocating ; and in particular the diagram of the curves for a voltaic current is, in my view, wrong. I cannot say I thought it wrong at the first glance ; I was pleased to see that Mr. Poynting held what I consider the correct view, that the main seat of the E.M.F. in a simple pile is at the zinc-acid junction and not at the zinc-copper, and that his diagram embodied this view.

But when I came to draw the curves myself for this case, taking account of the step of potential which I believe to exist between a metal and the air, I found the energy-paths refuse to start spreading out from the zinc-acid junction, as we had

* Poynting, Phil. Trans. 1884, pt. ii., "On the Transfer of Energy in the Electromagnetic Field."

both at first sight expected them to : they insisted on diverging from the zinc-copper junction, as a Voltaist would wish them to. This behaviour is really in perfect accord with my theory; though the fact is the cause of Voltaists' views concerning seat of E.M.F. They locate the force at the zinc-copper junction just *because* the energy-paths diverge thence and appear to start thence.

Prof. Poynting's diagram, therefore, not only differs from my theory, a thing which is natural and unimportant, but it is discrepant with experimental fact.

Rigid experimental truth would have led him to spread out his energy-paths from the metallic junction, not from the acid junction as he has done. True such a proceeding would have landed him in difficulties and apparent absurdity, and he would have been compelled to ask, "How can all the energy start from an inert metallic junction?" And he might well have considered such a conclusion absurd, as it is.

The reconciliation between sense and fact is simple. The lines do, indeed, diverge and spread out from this junction, but they do not *start* thence. They all start from the zinc-acid junction; but they at first creep along close to the surface of the zinc, investing it like the coats of an onion; and some continue to creep along the copper too, but some do not. More lines invest the zinc than the copper; and the lines in excess are given off at the junction, and thence diverge through the air.

An electroscopic experimenter seeking the seat of E.M.F. traces these lines, home as he thinks, to the zinc-copper junction, and hence regards this as their origin and the seat of the force. It is not. The lines do not start thence; they start at the real seat of E.M.F. and source of all the energy, though their close-lying continuations between metallic junction and acid junction are impossible to trace out experimentally.

With this preliminary, the diagram of energy-paths for a voltaic circuit in Plate IV. almost explains itself.

The lines are loci of constant electrostatic potential; and I happen to have chosen a circuit whose external resistance is twice its internal. The junction of the two metals is placed at random wherever its peculiarities may be most conveniently observed. The liquid in which the metals are immersed is only faintly indicated by the outline of its trough, and, for simplicity, it is supposed to be one differing in no electrostatic respect from air. Dilute sulphuric acid of a certain strength has been shown by Prof. Clifton to satisfy this condition almost exactly.

The particular metals for which the diagram is intended to

be drawn are zinc and copper. Now thermo-chemical data lead me to conclude that the step of potential between clean zinc and air is 1·8 volt [the exact decimal not being insisted on except for purposes of illustration] ; hence, if an equipotential line be drawn for every tenth part of a volt, 18 of these lines would closely invest clean zinc always, and form a boundary between it and the air. In the diagram (Pl. IV.) lines are only drawn for every fifth of a volt, and consequently 9 of them envelop the zinc.

As for copper, it is probable that the step of potential between it and air is ·8 of a volt ; and accordingly four lines invest the copper at every point in the diagram. They are not always *the same* four lines—that is of no consequence ; the essential thing to remember in drawing the diagram is to keep the number constant at every point for the same metal. Such a metal as gold or platinum may be nearly free from lines.

It is needless to say that the lines have to be drawn with spaces between them for distinctness, but that in reality the step of potential is of the most sudden description, and all investing lines ought to lie close to the mathematical surface of the metal.

Now, since nine lines coat the zinc, and only four the copper, it is manifest that five lines must leave the metallic surface at the junction, and must spread out into the air. These five lines correspond to the one volt* Volta effect observed near a junction of clean zinc and copper, and which has been called a difference of potential between zinc and copper.

Of the five lines thus starting to spread out from the junction, some make for points on the external circuit, and others go through the internal circuit. Those which go to the external circuit can by no means go through it, or into it, for the potential of the copper differs by ·8 volt from that of the air ; but they can go round it, and leave it on the other side just as if they had gone through it. Moreover, a line does go through, or into, the copper at the very point where the air-line abuts against it, viz. the line of potential ·8 volt lower. So the transfer of energy into the conductor of the external

* Different observers give different values for this number ; from ·5, Kohlrausch, to between ·76 and ·89, Ayrton and Perry and Clifton, up to ·92, Pellat ; but since tarnish or dirt on the zinc surface will certainly lower it, and it is difficult to suppose any zinc surface absolutely clean, I have not thought it necessary to modify the theoretical value of one volt, which at any rate serves well enough for illustration, though it may turn out to be about ten per cent. too high.

circuit is complete, except for just the metallic skin, and everything goes on *in the outer field* as if the energy-paths really started from both sides of the zinc-copper junction, and some of them cut through the metal of the external circuit with degradation into heat.

Note, however, that the heat-energy in a wire is not really thus carried through the air : it creeps along the surface of the wire, some of it perpetually diving into the wire, and becoming converted into heat.

The energy which liberates hydrogen from the copper-acid surface, moreover, does not reach it in the way Mr. Poynting has supposed. Some of it crawls over the current the whole distance ; all of it goes some distance with the current.

In the electrolyte, however, this is not so ; and the heat which there makes its appearance is produced at the expense of energy which has arrived *via* the air from the zinc-copper junction. This is on the supposition that no contact-force exists between the electrolyte surrounding the plates and the dielectric medium (air) enveloping the rest of the circuit. In cases where such a contact-force does exist, some energy will creep along the air-liquid surface, just as it does over the surface of a metal.

In the diagram one of the five lines here spoken of is doubled back upon itself, so as never to leave the surface of the zinc : the spaces between the lines are the real paths of the energy of course, not the lines themselves ; the lines only indicate the directions across which no energy goes. There are five spreading-out energy-paths, then, and it is most instructive to watch the energy arising at the zinc-acid surface flow in its first narrow channels, then broaden out through space, and then cramp itself again into its tube, by which it reaches the copper surface, and is transformed into energy of chemical separation ; or, again, to watch that in the channels touching the copper fritter itself gradually away into heat.

Energy-paths in Secondary Circuits. (Plate V.)

The four figures in Plate V. are intended to indicate the transfer of energy to a secondary circuit, *i. e.* one in which an induced current is being generated.

Fig. 1 shows a steady electromagnetic field disturbed by the presence of a stationary copper channel. The energy simply flows round it, as Prof. Poynting has said. Fig. 2 shows a field no longer steady, but with the potential changing : the energy-paths moving, say, toward the right. Each line, as it comes up,

folds itself round the circuit, and sends part of one of the previously investing paths to the inside, where it displaces part of one of the original inside paths, which at once begins to close up. Fig. 2 shows it half closed, on the hypothesis of absolute suddenness. Fig. 3 the same thing, on the supposition that energy possesses something like inertia. The induced current is produced by energy flowing along the semi-displaced path through the copper. If the change of field stops, fig. 2 is supposed quickly to revert to fig. 1, with an opposite transference of energy. Three stages in the change of field are shown in fig. 4: or this figure may be regarded as showing three lines in a uniform field disturbed by a moving conductor.

The arrows are drawn as if the energy flowed right away again without loss, or at least without complete loss. It is easy to reverse one of the arrows, so as to make energy flow up from both sides to the secondary circuit; but it is not easy to see how energy can reach the copper and be dissipated when its path does not pierce the copper at all. The lines have to be thought of as first embracing the circuit, then as moving away, having left a portion enclosing the circuit behind, and finally, more lines having come on, as sinking into and through the copper, and shutting themselves up; and so on perpetually, as long as the field is changing. The energy thus reaches the secondary circuit a little behindhand always, which is not wrong. If all the energy is dissipated in the act of passing through the copper, arrows ought not to be drawn on the inside paths which are shutting up; because though they are still paths for energy, it does not follow that they are made use of; in fact, they cannot be, if all the energy is extinct.

Probably, however, what all these diagrams better represent is the steady state of a field disturbed by the presence of a *perfect* conductor in which there is no dissipation whatever—a conductor like the molecules postulated in Ampère's and Weber's theory of magnetism and diamagnetism.

Voltaic Energy-paths. (Plate IV.)

Returning to the Plate showing voltaic-energy path: the diagram seems to me to constitute the most absolute and complete reconciliation between the truth in the opposing views of the voltaic circuit.

It not only justifies modern voltaic experimenters in their recognition of the slope of air-potential near a junction, but it partially justifies even the view of old Volta that this

Phil. Mag. S. 5. Vol. 19. No. 121. June 1885. 2 L

junction is the source of all the energy. So far as the external circuit is concerned, the metallic junction *is* the source of the energy ; all the energy does in a manner start thence, though it does not start *originally* thence. It reaches the junction from another place, its real source, but it travels thither by infinitely unrecognizable paths.

Does the diagram, then, justify modern Voltaists altogether? Certainly not. It cuts the ground from under the feet of those who have asserted that there is an E.M.F. at the junction of zinc and copper—a contact-force which is the cause of the observed difference of air-potential in the neighbourhood. And it annihilates those who have seen in this imaginary and non-existent force any means of propelling the current in a voltaic circuit.

On the other hand, the diagram of course justifies all those experimenters who have regarded the zinc-acid junction as the main seat of energy-transformation, and therefore of electromotive force. At the same time it exhibits the crudity of some of their statements ; and, just in so far as it upholds the truth and consistency of the Voltaists with regard to a part of the phenomenon, it contradicts some of the opposing statements made by chemical theorists with regard to the Volta effect. In particular, it lends no support to the customary “chemical” view, which sets down the whole Volta effect as an accident due to unavoidable chemical action, and so practically ignores it.

Imagine the lines investing the metals to be microscopically close to them, and then look at the diagram as truly exhibiting the facts of nature : it irresistibly suggests a voltaic or contact theory of the voltaic pile. It was right, therefore, for the first observers of voltaic phenomena to found a contact theory, and to adhere to it tenaciously, as in some sense true. It was also right for later philosophers, imbued with notions of energy, to feel the doctrine that the metallic junction was the source of power to be absurd and incomprehensible, and to found a chemical theory.

In the conflict both sides have made mistakes ; but it is the essence of truth in both views which has given them such vitality and endowed their upholders with such energy of persistence.

University College, Liverpool,
May 17, 1885.

LVI. *On the Thermoelectricity of Molten Metals.**By F. BRAUN (Carlsruhe)*.*

1. **I**N a circuit of metallic conductors a current produced by difference in temperature must be the equivalent of heat derived from an external source. If we assume (1) that the process is reversible, (2) that heat is absorbed only at the point of contact of higher temperature, and is given out only at the colder point of contact, then it follows that the thermoelectric force must be proportional to the difference in temperature of the junctions for intervals of any magnitude. If we assume a change in potential function at both junctions, the result may be expressed thus: that the change in potential is proportional to the absolute temperature of the junction. The electromotive force therefore, expressed as a function of the difference of temperature, ought to be a straight line. But experiment shows that in all the thermopiles as yet examined this linear relationship does not hold good; in by far the majority of cases the curve is much nearer a parabola. This led Sir W. Thomson to the conclusion that even in the interior of one and the same metal unequally heated there must be produced, upon the passage of a current, a quantity of heat proportional to the first power of the current-strength (adopting Le Roux's term, we may call this "the Thomson-effect").

In fact, if the thermopile works, not between two temperatures, but between an infinite number, it follows inversely that its electromotive force can no longer be expressed as a linear function of the greatest temperature-difference which occurs in it. Although, strictly speaking, nothing can be determined as to the locality of the electromotive force, yet we should be disposed to add to the changes in potential assumed to exist in the junctions (of which, however, no proof has yet been given that they occur there) other changes of potential occurring in the interior of the unequally heated metal.

Hitherto, so far as I know, only thermopiles have been examined in which at least one metal has been solid. The explanation is then obvious, that these internal thermoelectric forces are due to changes produced by the heat itself in the solid metal—hot and cold metal behaving like two different substances, or, as we may shortly express it, the heat produces structural changes. In fact, as shown by the

* Translated from the *Sitzungsber. der königl. Akad. d. Wissensch.* of Berlin, of April 9th, 1885.

experiments of Magnus, pieces of the same metal but of different temperature produce a thermoelectric current when brought quickly into contact. Only with lead could this result not be observed with certainty. But in lead, as Le Roux showed, the Thomson-effect is nearly or quite zero. In general, those metals which show a large Thomson-effect show also the further peculiarity that, upon heating a single wire the ends of which are kept at a constant temperature, a thermoelectric current is produced, the intensity (and even direction) of which often varies with insignificant changes in the distribution of the temperature or the structure of the wire.

These currents, I may at once mention, are so energetic, and especially so variable, particularly if the temperature is raised to a red heat, that I have had to take precautions with my thermopiles that there should not be any great differences of temperature in the solid metal. With lead only have I been unable to observe with certainty any such currents, even when the solid metal was plunged into molten lead. According to an experiment of Magnus's, often quoted, which for short I will call the Magnus experiment, currents are altogether absent in mercury.

It is an obvious explanation to refer the currents, produced on bringing together a cold and hot metal, to differences in structure (although we must then ascribe some influence either to time or to distribution of temperature), and to class them together with the Thomson effect—that is, the deviations which the thermoelectric force shows from proportionality with the temperature-difference. If certain metals give no thermo-current either in the Magnus experiment or when a wire, whose ends are maintained at a constant temperature, is heated, this may be explained in two ways: either changes of potential take place on both sides of the point heated, which, however, must decrease equally in both directions, or no thermoelectric excitation takes place, because, in consequence of the nature of the metal, differences in structure are impossible.

To this latter, although more special, assumption it seems to me we shall be most inclined. It is to be assumed that other molten metals will behave as mercury does, which, in fact, I have found to be the case (*cf.* § 7).

2. If we regard the Magnus experiment in this second way, it would follow that the thermoelectric force between molten metals must be proportional to the difference in absolute temperature between their points of contact. But this would show the possibility of measuring absolute tem-

peratures to a very high point by a method presenting many advantages over measurements made with an air-thermometer (smaller space of constant temperature, independence of expansion and changes in form of the vessel). We should further, and probably for the first time, obtain numbers which are entirely dependent upon the chemical nature of the substance, and free from uncertainties as to how hard or soft the body is. They would offer us a more ready conclusion as to the internal processes by which thermoelectric forces are excited. In fact, it appears very remarkable how, under similar external conditions (where any desired quantities of heat are available), large quantities of heat are transformed into the form of energy of mechanical work, at the points of contact of many metals, and with others very small quantities*.

Since, further, each molten metal in thermoelectric relationship must be characterized by a single constant (which no doubt may vary with the pressure), this must stand in some relationship to other constants, characterizing the particular metal, and independent of the temperature; but of such there is only the atomic weight available.

3. These considerations induced me to measure the thermoelectric forces of molten metals against each other between as wide limits of temperature as possible. A linear dependence upon the temperature appeared to me *à priori* so probable, that I directed my attention from the first to the utmost possible chemical purity of materials, and, moreover, chose only elements. Alloys were investigated only in the second place.

In the following I give the combinations which were measured, as well as the range of temperature employed; t denotes the temperature of the colder point of contact, T the highest reached at the warmer point of contact.

NaHg	$t=20^{\circ}$ or 100° ; $T=370^{\circ}$.
PbHg	$t=20^{\circ}$; $T=580^{\circ}$.
KHg	$t=20^{\circ}$ or 100° ; $T=520^{\circ}$.
SnHg	$t=20^{\circ}$; $T=460^{\circ}$.

* That we may regard the Peltierian heat as the equivalent of the work which the electricity does in passing to a lower potential-level at the points of contact, requires no justification. The work lost at the one contact-point can be restored by the equal amount gained at the other, just as the quantities of work in the portions of the two arms of a siphon at equal heights compensate each other. We cannot conclude anything, as Maxwell has done, as to the difference of potential produced, as a result of contact-electricity, from the Peltierian heat at the point of contact of two conductors.

Hg(Na + K)*	$t = 20^{\circ}$; $T = 380^{\circ}$.
PbSn	$t = 20^{\circ}$; $T = 435^{\circ}$; $t = 424^{\circ}$; $T = 974^{\circ}$.
BiSn	$t = 508^{\circ}$; $T = 882^{\circ}$.
Hg(Hg + Bi + Pb)†		$t = 20^{\circ}$; $T = 530^{\circ}$.

4. For the determination of high temperatures in spaces frequently small I employed spirals of platinum wire, whose galvanic resistance had previously been determined as a function of the temperature. I employed for this purpose a large muffle furnace heated by coal, the description of which, as well as of other experiments made with it, I propose to give shortly elsewhere. The vessels for the air-thermometer were made of Meissen porcelain ‡. The dimensions could be so chosen that, assuming constancy of temperature, temperatures of 1000° C. could be accurately measured to 1° or 2° .

5. The metals were enclosed in glass tubes, wherever glass could be used. Platinum wires melted into the glass served for external connections. With some metals the platinum wire was in direct contact with the metals; with others this contact was made by means of a pencil of retort-carbon § or graphite, with others by means of an iron-wire. Two glass tubes filled with metal were placed vertically side by side. At the hotter (upper) point of junction the platinum wires were melted together outside the tubes by means of the oxy-hydrogen blowpipe. So far as the wires which serve to make contact with the fluid metal at the hottest and coolest points penetrate the ends of the tubes, uniformity of temperature must be maintained. The cooler (lower) ends of the tubes were plunged into two concentric vessels full of mercury, which were placed in a bath of constant temperature, though not always the temperature of the room. From these mercury vessels wires of lead conducted to a petroleum bath of the temperature of the room, from which the copper connections were made. The leaden wires of course could not be placed directly in contact with the mercury. Their ends were melted into a glass tube, through the lower closed end of which a short platinum wire passed. I pass over here the precautions found necessary in order to ensure that the

* Alloy liquid at ordinary temperatures; about 1 eq. K to 1 eq. Na.

† Alloy liquid at ordinary temperatures; 1 part lead, 1 part bismuth, 3 parts mercury.

‡ I wish here to express my obligations to the Royal Saxon Porcelain Manufactory, and especially to M. Büttner, for the great trouble which they have taken in the very difficult manufacture of these vessels.

§ Platinum at 200° is amalgamated by mercury, and at about 500° is dissolved as energetically by mercury as it would be by boiling aqua regia.

other portions of the circuit should be affected only by slight thermoelectric forces, which remained constant for a considerable time and which were separately determined. The electromotive forces were measured by compensation.

6. The temperature of the hotter point of contact was maintained:—(1) In a bath of mineral oil of high boiling-point, which was kept agitated by a stirrer. In this way a temperature of about 380°C . could be obtained. (2) In an air-bath, consisting of three concentric iron cylinders, surrounded on the outside by a jacket of iron and chamotte, which was constructed according to my designs for another investigation, and which Ehrhardt* has recently described. It was heated by means of gas, and permitted the attainment of a temperature of 580° . (3) By a brisk current of vapour of constant boiling-point, sent through a glass vessel protected against external radiation of heat. The substances employed were alcohol 80°C ., water, xylol $140^{\circ}\cdot 5\text{C}$., aniline 183°C ., dimethylaniline 192°C ., toluidine 198°C ., xylidine 214°C . In order to decide with certainty particular questions I required constant temperatures, not differing much from each other. For the control of other measurements, I further employed baths of vapour of benzoic acid 250°C ., mercuric iodide 357°C ., and sulphur 448°C . For still higher temperatures (up to 980°C .) I employed (4) a muffle. The vessels which contained the metal were of earthenware. Imagine an earthenware tube inclined some 10° to the horizontal plane, and provided at both ends with vertical pipe-heads. Two such tubes of equal length were placed side by side. Each contained a molten metal. The upper pipe-heads were within the muffle, and heated to a higher temperature than the lower ones. They were connected by a bridge of thick iron-wire or of retort-carbon, by means of which a wide thick-walled chamotte-neck passed from the muffle through the heating-channel outwards, through which the earthenware tubes passed; the other two pipe-heads were outside the muffle, surrounded by an iron box, which was heated by coal. The temperature then was maintained as close as possible to the melting-point of the more difficultly fusible metal. In order to carry the circuit from the molten metal outside the muffle, previous experience had shown lead to be the only available metal which permitted the reduction of temperature to that of the room. The following arrangement was adopted in view of its properties:—At the lower end of a thin-walled brass-tube of about 12 millim. diameter an iron tube

* Wied. *Ann.* vol. xxiv. p. 217 (1885).

was brazed ; it was closed below by an iron disk screwed on, to the inside of which a piece of platinum foil was soldered. The screw closed against the molten metal in which the iron tube plunged. A platinum wire was in contact with the platinum foil. This was melted through the closed lower end of a glass tube. The bottom of this was filled to a height of about 10 millim. with graphite. Lead wire was pushed slowly into the glass tube, which melted and formed a continuous connection from the molten to the solid lead. The solid lead wire conducts again to points at the temperature of the room. All the different conductors (iron, platinum, and graphite) were completely surrounded by the molten metal in the pipe-head, which stood some 4 centim. above the top of the graphite layer. We may therefore assume equality of temperature in all. This last-described arrangement was employed for the circuits PbSn and SnBi.

7. I pass now to the results obtained. If we take the temperatures as abscissæ and the electromotive forces as ordinates, the following results are obtained. Most, if not all, the curves are not actually straight lines ; they are of such a form that the centre of curvature lies on the side of the curve turned away from the axis of abscissæ, *i. e.* the electromotive forces increase more rapidly than corresponds to proportionality with the temperature. Only the curve for NaHg, which is almost straight for a considerable distance, seems at higher temperatures to assume a curvature towards the other side. The thermoelectric force of this element is, however, so small that measurements with it offered special difficulties. KHg is almost a straight line, but possesses certain peculiarities (see below).

The curves are certainly not of the second degree, but of the third at least. Tait's assumption that specific heat of electricity is proportional to the absolute temperature, which would lead to the equation to a parabola, therefore does not hold good for molten metals. I give here a few numbers from the curve for the element PbHg. It is of especial interest, on account of the property of lead, not to give any Thomson-effect. The first column gives the temperature of the warmer point of contact in Centigrade degrees, the second the same reckoned from absolute zero. The temperature of the other point of contact is throughout 20° C. The third column contains the electromotive force e in micro-volts. If e is a function of θ , represented by a curve of the second degree, then $\frac{de}{d\theta}$ ought to be a linear function of θ . The fourth column shows the

TABLE I.
Lead-Mercury.

	θ .	ρ .	$\frac{de}{d\theta}$.		Π calculated.
			Observed.	Calculated.	
20° C.	293	0	3.4	...	46.2
100	373	340	4.8	...	13.0
200	473	920	7.0	6.5	153.7
300	573	1710	9.2	8.3	244.6
400	673	2640	10.2	10.0	318.5
500	773	3750	12.5	11.8	448.3
580	853	4940	19.0	13.2	751.9

observed value of $\frac{de}{d\theta}$, and the fifth the value calculated from the first two numbers of the fourth column. The differences are far greater than possible errors of observation. The last column gives the quantities of heat in gramme-calories which result at the point of contact of absolute temperature θ , when the quantity of electricity electro-chemically equivalent to 2 grammes hydrogen (193,000 coulombs) passes through.

If we conclude from the experiments of Le Roux (who, however, gives no absolute value of the Thomson-effect) that there is no Thomson-effect in lead, there must be such in mercury. Moreover its magnitude must be considerable. This follows from the observations with the couples HgCu, HgPt, HgFe, whose electromotive force I have measured for considerable differences of temperature. The Thomson-effect is very considerable in the metals Cu, Pt, Fe; in mercury it must be at least of equal magnitude according to the results of these measurements.

The wires employed of the three metals when heated in the middle, whilst the ends were maintained at constant temperature, gave very considerable electromotive forces (70 to 200 microvolts) even after they had been repeatedly heated. Mercury on the other hand, as is well known, gives none. This last phenomenon, and the Thomson-effect, cannot therefore stand in any direct connection.

In the thermo-element PbHg, the contact-points may have the respective absolute temperatures Θ and θ ($\Theta > \theta$); it may be supposed to remain closed so long that its thermo-current has conveyed the electro-chemical unit of electricity through it. Then at the temperature Θ the quantity of heat Π_{Θ} is absorbed, and at θ the quantity of heat Π_{θ} is given off to surrounding objects. The difference $\Pi_{\Theta} - \Pi_{\theta}$ in the present

example cannot be completely converted into electrical energy (with an oppositely bent curve it would not be the only equivalent), but there would be also heat from the interior of the metals themselves converted into work.

The following Table II. gives for different intervals of temperature the heat $\Pi_{\Theta} - \Pi_{\theta}$ in gramme-calories. Further, the electrical work L in the same units. The result is that in the most favourable case 47 per cent. of the heat $\Pi_{\Theta} - \Pi_{\theta}$ appears as useful electrical work. The remainder, at least 53 per cent., remains in the metals themselves, in the form of (reversible) heat. It is given in the fourth column.

The thermopile absorbs the quantity of heat Π_{Θ} at the highest temperature Θ ; at least, the pile works with this heat-capital. How much of this heat appears as useful work? The sixth column shows that it is at the outside 40 per cent. If of the quantity of heat Π_{Θ} the whole remainder not converted into work at the temperature θ were given off, the fraction $\frac{\Theta - \theta}{\Theta}$ must be converted into current-energy. The next column shows this fraction.

TABLE II.

Temp. interval.	$\Pi_{\Theta} - \Pi_{\theta}$.	Elec. work, L.	$\Pi_{\Theta} - \Pi_{\theta} - L$, or Heat in metals.	$\frac{L}{\Pi_{\Theta} - \Pi_{\theta}}$.	$\frac{L}{\Pi_{\Theta}}$.	$\frac{\Theta - \theta}{\Theta}$.	$\frac{\Pi_{\Theta} - \Pi_{\theta}}{\Pi_{\Theta}}$.
20-100	36.8	15.8	21.0	0.43	0.19	0.21	0.44
20-200	107.5	42.7	64.8	0.40	0.28	0.38	0.70
20-300	198.4	79.4	119.0	0.40	0.32	0.49	0.81
20-400	272.3	128.5	149.8	0.47	0.40	0.56	0.85
20-500	402.1	174.0	228.1	0.43	0.39	0.62	0.89
20-600	705.7	229.2	476.5	0.32	0.30	0.66	0.94

If we now enquire whether thermo-elements are suitable arrangements for converting heat into work, we obtain the following result:—Elements for which the electromotive force increases greatly with rise of temperature, as is the case for the element HgPb, work with very small useful effect (not regarding the loss of heat by conduction, radiation, &c.). Thermo-elements whose electromotive force decreases with rise of temperature work with more useful effect, but contribute absolutely less towards the work to be done.

Other elements with liquid metals give essentially the same results as the PbHg element. The curve for Hg(Hg + Br + Pb) has nearly the same form. In this combination both constituents are fluid at ordinary temperatures; I have examined them for an interval of more than 500°. It appeared to me

of particular interest to repeat Magnus's experiment with this fluid alloy. If a portion of the amalgam was plunged cold into the rest heated to about 300° , no thermo-current was produced the electromotive force of which exceeded 0.5 microvolts, which was the lowest I was in a position to measure.

The curve for this element shows a remarkable peculiarity. At 180° it is convex towards the axis of abscissæ, but between 180° and 210° it becomes concave, and then again convex. In this respect it is qualitatively exactly like the curve which platinum-iron elements give, only that in the latter the peculiarity is more decided. With a PtFe element the curve rises steeply to about 360° and somewhat concave towards the axis of abscissæ, then bends rapidly towards the horizontal direction, having a maximum at 420° , then falls from this very little (almost horizontal) to a minimum at 520° , rises then slowly, and then from about 630° ascends again to 1000° as steeply as from 0° to 36° .

The element consisting of molten potassium and mercury shows an exactly similar behaviour, the curve rising with oscillations. Consistent results are, however, only obtained with this latter element when vapours of constant boiling-point are employed as source of heat. Using the oil-bath or the air-bath, the values obtained at the same temperature often vary considerably.

We shall probably only be able to explain these phenomena by the assumption that molecular transformations take place also in liquid bodies (as is known to be the case, for example, with sulphur); and these changes appear, at least as far as their velocity is concerned, to depend on the rate of heating. The question then suggests itself whether molten cadmium, which in the form of vapour is monatomic, combined with a thermoelement with the similarly constituted mercury, would show linear dependence of electromotive force upon temperature.

The interval of temperature available seemed to be too small to determine this point with certainty; and I considered it beside the mark to employ cadmium amalgam, since we could no longer assume the existence of isolated atoms.

8. Taking all the results together, we may come to the conclusion that in thermoelectricity we are still further from an insight into the true nature of things than most physicists have supposed. We must assume the existence of electromotive forces even in the interior of molten metals of unequal temperature; and, moreover, these follow no simple laws. It would not be difficult to find formulæ for the electromotive forces supposed to exist in the interior of metals which should satisfy the observations; but it seems to me

that we do not thereby come nearer to a representation of the processes which occur. We may say with certainty that fluid unequally-heated metals are electrically charged, and that by simple inequalities of temperature in a conductor the conditions for transformation of heat into work are fulfilled, only that we cannot obtain the work in the convenient form of a closed current. Perhaps this excitation of electricity takes place in all fluids, and generally in all substances, *i. e.* also in the so-called vacuum; and the further question, important in principle, may arise, what influence this hitherto disregarded circumstance has upon physical processes.

If we return to the facts detailed above, everything indicates that thermoelectric excitement is an intermolecular process dependent upon the number or arrangement of atoms in the molecule. It is thus suggested that all the rough mechanical changes, which exert so much influence on the thermoelectric behaviour of a solid body, and which consist in drawing, bending, hardening, annealing, &c., are also connected with intermolecular, and thus in a measure chemical, changes. This would be in harmony with the one already known fact, that in steel, upon passage from the soft into the hard condition and *vice versa* (also by simple drawing), the quantity of chemically combined carbon changes. Hard and soft steel give, as is well known, a tolerably powerful thermoelectric action.

LVII. *On the Expression for the Complete Elliptic Integral of the Second Kind as a Series proceeding by Sines of Multiples of the Modular Angle.* By J. W. L. GLAISHER, M.A., F.R.S.*

Series for K and E, §§ 1-4.

§ 1. **I**N vol. xix. pp. 51, 52 of Crelle's Journal, Gudermann has given the following interesting expression for K in a series proceeding by multiples of the modular angle θ :—

$$\frac{K}{\pi} = \sin \theta + \frac{1^2}{2^2} \sin 5\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 9\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 13\theta + \&c. \quad (1)$$

The coefficients are the same as in the well-known fundamental formula

$$\frac{2K}{\pi} = 1 + \frac{1^2}{2^2} \sin^2 \theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin^4 \theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin^6 \theta + \&c. \quad (2)$$

* Communicated by the Author.

Of course for the actual calculation of the numerical value of K corresponding to a given value of θ , the second formula is much to be preferred; for the trigonometrical factors diminish rapidly, whereas in the first series they have no tendency to diminish, and the gradual convergence of the series is due solely to the numerical coefficients. Thus in (1) the n th term is of the order $\frac{1}{n}$; but the corresponding term in (2) is of the order $\frac{1}{n} \sin^{2n-2}\theta$. When $\theta=0$, the series in (1) is of the form $0 \times \infty$, the true value being $\frac{1}{2}$; when $\theta=\frac{1}{2}\pi$, the series is infinite in value, as it should be.

§ 2. Gudermann's process is in effect as follows. He shows that the transformation which converts k into $e^{-2i\theta'}$ converts K into $\frac{1}{2}e^{i\theta'}(K-iK')$, where θ' denotes $\frac{1}{2}\pi-\theta$. Starting with (2), he thus finds

$$\frac{K-iK'}{\pi} = e^{-i\theta'} + \frac{1^2}{2^2}e^{-5i\theta'} + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2}e^{-9i\theta'} + \&c.,$$

from which (1) is immediately deducible by equating the real parts.

At the end of the investigation Gudermann, after referring to the slow convergence of (1), which renders it of no practical value, adds:—"Aus diesem Grunde übergehen wir auch die Herleitung einer ähnlichen Reihe für den Quadranten E."

The main object of this note is to give the series for E which corresponds to (1). It does not admit of quite such simple derivation as the series for K .

§ 3. Gudermann's transformation corresponds to the change of q into $iq^{\frac{1}{2}}$; and this change may be supposed to be produced by the change of q into $q^{\frac{1}{2}}$, followed by the change of q into $-q$.

By the change of q into $iq^{\frac{1}{2}}$, k' is changed into $e^{-2i\theta}$, and K' is changed into $\frac{1}{2}e^{i\theta}(K'-iK)$. Starting with the series

$$\frac{2K'}{\pi} = 1 + \frac{1^2}{2^2}k'^2 + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2}k'^4 + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2}k'^6 + \&c.,$$

we thus find

$$\frac{K'-iK}{\pi} = e^{-i\theta} + \frac{1^2}{2^2}e^{-5i\theta} + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2}e^{-9i\theta} + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2}e^{-13i\theta} + \&c.$$

§ 4. In order to apply the same process to the series for E' , viz.

$$\frac{2E'}{\pi} = 1 - \frac{1}{2^2}k'^2 - \frac{1^2 \cdot 3}{2^2 \cdot 4^2}k'^4 - \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6^2}k'^6 - \&c.,$$

it is necessary to determine the quantity into which E' is converted by the change of q into $iq^{\frac{1}{2}}$.

It can be shown that, by the change of q into $q^{\frac{1}{2}}$,

$$E' \text{ becomes } \frac{E' + kK'}{1 + k};$$

and that, by the change of q into $-q$,

$$k \text{ becomes } \frac{ik}{k'},$$

$$K' \quad ,, \quad k'(K' - iK),$$

$$E' \quad ,, \quad \frac{E' - k^2 K' + i(E - k'^2 K)}{k'}.$$

Thus, by the change of q into $iq^{\frac{1}{2}}$, E' becomes

$$e^{-i\theta} \{E' - k^2 K' + k k' K + i(E - k'^2 K + k k' K')\}.$$

It follows, therefore, that

$$\begin{aligned} & \frac{2\{E' - k^2 K' + k k' K + i(E - k'^2 K + k k' K')\}}{\pi} \\ &= e^{i\theta} - \frac{1}{2^2} e^{-3i\theta} - \frac{1^2 \cdot 3}{2^2 \cdot 4^2} e^{-7i\theta} - \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6^2} e^{-11i\theta} - \&c.; \end{aligned}$$

whence, equating imaginary parts,

$$\frac{2(E - k'^2 K + k k' K')}{\pi} = \sin \theta + \frac{1}{2^2} \sin 3\theta + \frac{1^2 \cdot 3}{2^2 \cdot 4^2} \sin 7\theta + \&c.$$

The left-hand member of this equation

$$= \frac{2E - K}{\pi} + \frac{K' \sin 2\theta - K \cos 2\theta}{\pi};$$

and therefore the equation itself may be written

$$\begin{aligned} \frac{2E}{\pi} &= \frac{K}{\pi} + \frac{K \cos 2\theta - K' \sin 2\theta}{\pi} \\ &+ \sin \theta + \frac{1}{2^2} \sin 3\theta + \frac{1^2 \cdot 3}{2^2 \cdot 4^2} \sin 7\theta + \&c. \end{aligned}$$

Now

$$\frac{K}{\pi} = \sin \theta + \frac{1^2}{2^2} \sin 5\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 9\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 13\theta + \&c.,$$

$$\frac{K'}{\pi} = \cos \theta + \frac{1^2}{2^2} \cos 5\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \cos 9\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \cos 13\theta + \&c.;$$

whence

$$\frac{K \cos 2\theta - K' \sin 2\theta}{\pi}$$

$$= -\sin \theta + \frac{1^2}{2^2} \sin 3\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 7\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 11\theta + \&c.$$

Thus

$$\frac{2E}{\pi} = \frac{K}{\pi} + \frac{1}{2} \sin 3\theta + \frac{1^2 \cdot 3}{2^2 \cdot 4} \sin 7\theta + \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} \sin 11\theta + \&c.;$$

or, substituting for $\frac{K}{\pi}$ its value,

$$\begin{aligned} \frac{2E}{\pi} = & \sin \theta + \frac{1}{2} \sin 3\theta + \frac{1^2}{2^2} \sin 5\theta + \frac{1^2 \cdot 3}{2^2 \cdot 4} \sin 7\theta \\ & + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 9\theta + \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} \sin 11\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 13\theta + \&c., \end{aligned}$$

which is the series for E corresponding to (1).

Formulae for K, E, I, U, V, W, §§ 5-9.

§ 5. The results obtained in the previous sections form part of a general system of formulæ which may be stated as follows :—

Consider two quantities I and G defined by the equations

$$I = E - K,$$

$$G = E - k'^2 K;$$

and let U, V, W denote

$$\frac{1}{2}(I + G), \quad \frac{1}{2}(G + E), \quad \frac{1}{2}(E + I)$$

respectively.

The values of the six quantities, expressed in terms of E and K, and of I and K, are therefore

$$E = E \qquad \qquad \qquad = I + K,$$

$$I = E - K \qquad \qquad \qquad = I,$$

$$G = E - k'^2 K \qquad \qquad \qquad = I + k^2 K,$$

$$U = E - \frac{1}{2}(1 + k'^2)K \qquad \qquad \qquad = I + \frac{1}{2}k^2 K,$$

$$V = E - \frac{1}{2}k'^2 K \qquad \qquad \qquad = I + \frac{1}{2}(1 + k^2)K,$$

$$W = E - \frac{1}{2}K \qquad \qquad \qquad = I + \frac{1}{2}K.$$

It can be shown that

$$\begin{aligned}\frac{2K}{\pi} &= 1 + \frac{1^2}{2^2} k^2 + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} k^4 + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} k^6 + \frac{1^2 \cdot 3^2 \cdot 5^2 \cdot 7^2}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8^2} k^8 + \&c., \\ \frac{2E}{\pi} &= 1 - \frac{1}{2^2} k^2 - \frac{1^2 \cdot 3}{2^2 \cdot 4^2} k^4 - \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6^2} k^6 - \frac{1^2 \cdot 3^2 \cdot 5^2 \cdot 7}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8^2} k^8 - \&c., \\ \frac{2I}{\pi} &= -\frac{1}{2} k^2 - \frac{1^2 \cdot 3}{2^2 \cdot 4} k^4 - \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} k^6 - \frac{1^2 \cdot 3^2 \cdot 5^2 \cdot 7}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8} k^8 - \&c., \\ \frac{2G}{\pi} &= \frac{1}{2} k^2 + \frac{1^2}{2^2 \cdot 4} k^4 + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2 \cdot 6} k^6 + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8} k^8 + \&c., \\ \frac{4U}{\pi} &= -\frac{1^2}{2 \cdot 4} k^4 - \frac{1^2 \cdot 3^2}{2^2 \cdot 4 \cdot 6} k^6 - \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6 \cdot 8} k^8 - \&c., \\ \frac{4V}{\pi} &= 1 + \frac{1}{2^2} k^2 + \frac{1^2}{2^2 \cdot 4^2} k^4 + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2 \cdot 6^2} k^6 + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2 \cdot 8^2} k^8 + \&c., \\ \frac{4W}{\pi} &= 1 - \frac{1 \cdot 3}{2^2} k^2 - \frac{1^2 \cdot 3 \cdot 5}{2^2 \cdot 4^2} k^4 - \frac{1^2 \cdot 3^2 \cdot 5 \cdot 7}{2^2 \cdot 4^2 \cdot 6^2} k^6 - \&c.\end{aligned}$$

§ 6. Denoting by accented letters the same functions of k'^2 that the unaccented letters are of k^2 , it can also be shown that, by the change of q into $q^{\frac{1}{2}}$,

$$\begin{aligned}k' &\text{ becomes } e^{-2i\theta}, \\ K' &,, \quad \frac{1}{2}e^{i\theta}(K' - iK), \\ I' &,, \quad e^{-i\theta}(W' + iW), \\ V' &,, \quad e^{-i\theta}(G' + iG); \end{aligned}$$

and

$$\begin{aligned}E' &\text{ becomes } e^{-i\theta}\{G' + kk'K + i(G + kk'K')\}, \\ G' &,, \quad e^{-i\theta}\{G' - kk'K + i(G - kk'K')\}, \\ U' &,, \quad \frac{1}{2}e^{-i\theta}\{G' + W' - kk'K + i(G + W - kk'K')\}, \\ W' &,, \quad \frac{1}{2}e^{-i\theta}\{G' + W' + kk'K + i(G + W + kk'K')\}.\end{aligned}$$

§ 7. Starting with the series for $\frac{2K'}{\pi}$, $\frac{2I'}{\pi}$, $\frac{2V'}{\pi}$, in powers of k'^2 , and changing k' into $e^{-2i\theta}$, we find, by equating the real, and the imaginary, parts in the resulting formulæ,

$$\begin{aligned}\frac{K}{\pi} &= \sin \theta + \frac{1^2}{2^2} \sin 5\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 9\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 13\theta + \&c., \\ \frac{K'}{\pi} &= \cos \theta + \frac{1^2}{2^2} \cos 5\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \cos 9\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \cos 13\theta + \&c., \\ \frac{2W}{\pi} &= \frac{1}{2} \sin 3\theta + \frac{1^2 \cdot 3}{2^2 \cdot 4} \sin 7\theta + \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} \sin 11\theta + \&c.,\end{aligned}$$

$$\frac{2W'}{\pi} = -\frac{1}{2} \cos 3\theta - \frac{1^2 \cdot 3}{2^2 \cdot 4} \cos 7\theta - \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} \cos 11\theta - \&c.,$$

$$\frac{4G}{\pi} = \sin \theta - \frac{1}{2^2} \sin 3\theta - \frac{1^2}{2^2 \cdot 4^2} \sin 7\theta - \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 11\theta - \&c.,$$

$$\frac{4G'}{\pi} = \cos \theta + \frac{1}{2^2} \cos 3\theta + \frac{1^2}{2^2 \cdot 4^2} \cos 7\theta + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2 \cdot 6^2} \cos 11\theta + \&c.,$$

§ 8. It appears from § 5 that the quantities I' and V' resemble K' in being transformable, by the change of modulus from k' to $e^{-2i\theta}$, into expressions of the form $e^{\pm i\theta}(P' \mp iP)$, where P' is the same function of k'^2 that P is of k^2 . Perhaps the simplest method of obtaining the sine-series for E is by deducing the sine-series for W from the k' -series for I' and then adding the sine-series for $\frac{1}{2}K$; and it was in this manner that I was first led to the series for E^* . It will be noticed that the process followed in § 4 also gives in the first instance the sine-series for $E - \frac{1}{2}K$ or W , and that the series for E is deduced from it by the addition of that for $\frac{1}{2}K$.

By the change of k' into $e^{-2i\theta}$,

$$k'K' \text{ becomes } \frac{1}{2}e^{-i\theta}(K' - iK),$$

and therefore

$$I' - k'K' \text{ becomes } e^{-i\theta}(I' + iE).$$

If then we start with the k' -series for $I' - k'K'$ and transform k' into $e^{-2i\theta}$, we obtain directly the sine-series for E by equating the imaginary parts in the resulting formula. The transformation therefore (of k' into $e^{-2i\theta}$) which converts

$$K' \text{ into } \frac{1}{2}e^{i\theta}(K' - iK)$$

converts also

$$K' - E' + k'K' \text{ into } e^{-i\theta}(K' - E' - iE);$$

and we may therefore derive the sine-series for E from the k' -series for $K' - E' + k'K'$ by a process exactly analogous to that by which the sine-series for K is derivable from the k' -series for K' .

The six quantities E, I, G, U, V, W are considered in detail in the Quarterly Journal of Mathematics, vol. xx. pp. 313-361, and in the Proceedings of the Cambridge Philosophical Society, vol. v. pp. 184-208. The formulæ in these papers show that E forms one of a triad of corresponding fundamental quantities of which the other two members are I and G .

§ 9. It may be remarked that $I = -J$, where J is the

* Proc. Camb. Phil. Soc. vol. v. p. 204.

quantity so denoted by Weierstrass, and $=K-E$. We thus have

$$\begin{aligned}\frac{2E}{\pi} &= \sin \theta + \frac{1}{2} \sin 3\theta + \frac{1^2}{2^2} \sin 5\theta + \frac{1^2 \cdot 3}{2^2 \cdot 4} \sin 7\theta \\ &\quad + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 9\theta + \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} \sin 11\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 13\theta + \&c., \\ \frac{2J}{\pi} &= \sin \theta - \frac{1}{2} \sin 3\theta + \frac{1^2}{2^2} \sin 5\theta - \frac{1^2 \cdot 3}{2^2 \cdot 4} \sin 7\theta \\ &\quad + \frac{1^2 \cdot 3^2}{2^2 \cdot 4^2} \sin 9\theta - \frac{1^2 \cdot 3^2 \cdot 5}{2^2 \cdot 4^2 \cdot 6} \sin 11\theta + \frac{1^2 \cdot 3^2 \cdot 5^2}{2^2 \cdot 4^2 \cdot 6^2} \sin 13\theta - \&c.\end{aligned}$$

LVIII. Notices respecting New Books.

An Elementary Treatise on Dynamics, containing Applications to Thermodynamics. By B. WILLIAMSON, M.A., &c., and F. A. TARLETON, LL.D. London: Longmans, Green, and Co. 459 pp.

THIS is unquestionably an important work on Dynamics, comprising within the compass of 459 pages the Dynamics of a Particle (218 pp.), the Kinematics and Kinetics of a Rigid Body (207 pp.), including two chapters on Energy (40 pp.) and one on Small Oscillations (43 pp.), and a final chapter on Thermodynamics (29 pp.). Except for a few applications to elastic material and some of the thermodynamics of gases (as illustrations of the Theory of Energy), the work is confined to the Motion of Rigid Bodies, and is on the whole a most carefully thought-out treatise on so large a subject.

It professes to "start from the most elementary conceptions, so that any student who is acquainted with the Calculus can commence the Treatise without requiring the previous study of any other work on the subject" (see preface). It is perhaps a natural result of this that the early parts are easy and in considerable detail, whilst the later parts are difficult from their conciseness. It may, indeed, be doubted whether any student could really read this Treatise through (and work out the examples) without external help in the later parts. The examples have been chosen with great care so as to form excellent illustrations of the text; of a great many, however, it must be said that they are rather instructive problems which have been solved by accomplished mathematicians than exercises for students; more or less full details of the working of these are given:—*e.g.* Ex. 15 to 27, on Kinetics of a Rigid Body, are "the substance of Prof. MacCullagh's Lectures on Rotation."

It is singular that though the work bears the names of two mathematicians on the titlepage, and seems to have received at least partial revision from a third (see preface), a decided looseness

of expression occurs here and there. Thus the opening lines run:—

“We give the name of matter to that which we regard as the permanent cause of any of our sensations.”

This definition would surely include heat, light, railway accidents, &c. Again the term *velocity* nowadays has surely the unique definite meaning of “space described (or capable of description) in a time-unit;” but the term is used in several places as if it had a simply spatial meaning, *e. g.* (p. 28) “a velocity of 400 feet,” (p. 29) “the velocity per minute,” and similar instances in many other places.

ALLAN CUNNINGHAM, *Major, R.E.*

Energy and Motion. A Textbook of Elementary Mechanics.

By W. PAICE, M.A. Cassell and Co. (Pp. 114.)

THIS is one more of the legion of small textbooks of elementary science the *raison d'être* of which it is hard to discover. The preface states that there “appear to be no good books on the subject which approach it from a sufficiently elementary standpoint;” but we fear that this book put into the hands of ordinary schoolboys or schoolgirls, unless supplemented by good oral teaching, will make too large demands upon their intelligence. There are a few good points of more or less novelty to be noted, but these are more than counterbalanced by erroneous teaching upon certain important matters. A few instances will suffice. The specification of an acceleration as “ $2\frac{1}{2}$ feet per second per second,” or as “24,000 yards per minute per minute,” is no doubt to be commended; but such unusual accuracy contrasts, unfortunately, with such statements as that “1 horse-power is equal to 550 foot-pounds” (p. 61); or that “to produce a velocity v in m units of mass, or to stop the mass m if moving with a velocity v , would require mv units of force” (p. 44).

The author seems to be of opinion that the formula $s = \frac{1}{2}ft.^2$ does not admit of rigorous proof, but always involves some assumption. Certainly he makes an assumption (p. 13); but using the word “therefore,” he gives his readers no hint of this when he says, “When a moving body is uniformly accelerated the increase of velocity is the same throughout for every equal small portion of time, *therefore* the distance passed over will be the same as if the body moved uniformly with the average velocity for the whole number of small spaces of time—that is, for the whole time.”

The statement on p. 39, “If a bullet of lead and another of wood of the same size be thrown at the same time with the same velocity, the bullet of lead will go further than the bullet of wood” needs no comment; it is sufficient to quote it. The meaning of “mass” is not explained by saying that there is more of “something” in a leaden bullet than in a wooden bullet of equal size; and the statement that there is more “matter” or “stuff” in a cubic inch of gold than in a cubic inch of water has simply no meaning at all. Until it is proved that gold is a compound of hydrogen and oxygen, in the proportions in which they occur in

water (which is *possible*, but extremely improbable), we shall not know which of these two contains most matter; and in the present state of chemical science it is simply idle to pretend to compare the quantities of matter in two things of different chemical nature.

We fail to see why a gramme should be called a "mass-gramme," and it is certainly *not* "the quantity of matter contained in a cubic centimetre of distilled water at the temperature and *pressure* at which water has its greatest density."

Until these and similar errors are corrected no competent teacher will consent to employ this production as a textbook.

LIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 393.]

April 29, 1885.—Prof. T. G. Bonney, D.Sc., LL.D., F.R.S.,
President, in the Chair.

THE following communications were read:—

1. On the Structure of the Ambulacra of some Fossil Genera and Species of Regular Echinoidea." By Prof. P. Martin Duncan, M.B. (Lond.), F.R.S., V.P. Linn. Soc., F.G.S.

2. "The Glacial Period in Australia." By R. von Lendenfeld, Ph.D. Communicated by W. T. Blanford, LL.D., F.R.S., Sec. G.S.

Although several previous writers have suggested that boulders and gravels found in different parts of Australia are of glacial origin, the evidence is vague, and no clear proof of glaciation has been brought forward. During a recent ascent of the highest ranges in Australia, parts of the Australian Alps, the author succeeded in discovering a peak which he named Mount Clarke, 7256 feet high, and in finding traces of glaciation in the form of *roches moutonnées* throughout an area of about 100 square miles. The best-preserved of the ice-worn surfaces were found in a valley named by the author the Wilkinson Valley, running from N.E. to S.W., immediately south of Müller's Peak and the Abbot Range. No traces of ice-action were found at less than 5800 feet above the sea.

The rocks showing ice-action are all granitic, and the fact that the surfaces have been polished by glaciers is said to be proved by the great size of such surfaces, by their occurrence on spurs and projecting points, by many of them being worn down to the same general level, and by their not coinciding in direction with the joints that traverse the rock.

In conclusion the author briefly compared the evidence of glacial action in Australia with that in New Zealand.

3. "The Physical Conditions involved in the Injection, Extrusion, and Cooling of Igneous Matter." By H. J. Johnston-Lavis, M.D., F.G.S., &c.

The great disproportion between the displays of volcanic activity

in the same volcano at different times, and between the eruptions of different volcanoes, is a subject deserving the most attentive consideration. The violence of a volcanic outburst does not bear any relation to the quantity of material ejected. The union of water with lavas may be compared with the solution of a gas in water; but there is reason to believe that in their deep-seated sources lavas contain little or no water. If igneous matter be extruded through dry strata the eruption might take place without explosive manifestations. But if igneous matter be extruded through water-bearing beds, a kind of dialysis would take place between the igneous and aqueous masses. In this way the tension of the steam in the fluid rock may at last become so great that a fissure will be formed at the surface and volcanic action will follow.

In this way the violence of a volcanic eruption will be determined by the quantity of water contained in the strata through which the lava passes in its passage to the surface, and by the temperature at which it reaches the surface.

This theory explains the acknowledged sequence of volcanic outbursts of different degrees of violence, and the intervals which occur between them. It also explains the differences between the central and lateral eruptions of a great volcano, and the phenomena attending its extinction.

The structures of the igneous rocks, whether of basic or acid composition, are greatly modified by the presence in them of volatile ingredients.

The succession of events indicated by the structure of Monte Somma and Vesuvius, Roccamonfina, Monte Vulture and Monte Nuovo, show that after a long cessation of volcanic activity we have an extensive production of fragmentary and scoriaceous material, and that this is gradually succeeded by the eruption of lava-streams.

The water and other volatile substances, such as sulphates and chlorides, which are given off abundantly in volcanic eruptions, may act as solvents for the various minerals which constitute lavas.

LX. Intelligence and Miscellaneous Articles.

APPLICATION OF PHOTOGRAPHY TO ELECTRICAL MEASUREMENTS.

BY JOHN TROWBRIDGE AND HAMMOND VINTON HAYES.

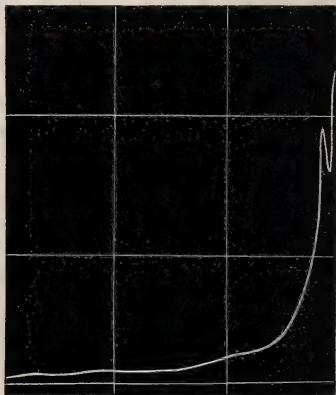
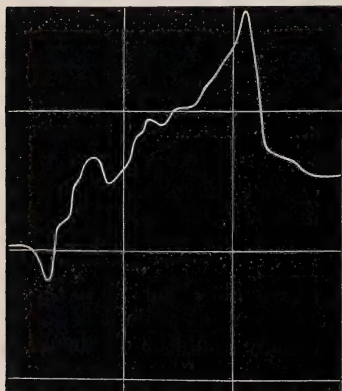
IN the study of electromotive force and of voltaic cells it is often desirable to have long-continued observations. The complete history of the action, for instance, of the Daniell cell with different strengths of solution extending over hours or days, if it could be presented to the eye as a curve, would be valuable to those who desire to know the behaviour of such a cell while it is doing work under definite conditions. Such curve could be obtained by patient observation, but it would be unprofitable labour for one to spend his time in watching the excursions of a galvanometer-needle, if the needle can be made to record its movements by any device.

The method we have used enables one to study the action of a cell at one's own leisure, the apparatus running at night or during the day when one is occupied with other work. A beam of light from a gas-flame passes through a vertical slit placed in front of the flame, and is reflected from the concave mirror of a tangent-galvanometer, of few turns of wire, through a horizontal slit, in a dark box, in which a sheet of sensitive paper is placed. By means of this arrangement of a vertical and a horizontal slit, a small point of light is obtained. A stationary concave mirror is placed near the needle of the tangent-galvanometer, so that the same beam of light may be reflected by both this mirror and the one attached to the galvanometer-needle. The spot of light given by the stationary mirror serves to mark the zero point of the needle when no current is passing through the galvanometer. The photographic paper is placed in a slide which is lowered uniformly by the unwinding of a string from a little cylinder placed either upon the hour-hand or the minute-hand of a cheap eight-day clock. When the electrical current from the voltaic combination, which is being used, passes through the galvanometer, its changes in strength for different times are indicated by the relations of the two lines drawn upon the sensitive paper. The line drawn by the light from the stationary mirror is a straight one, and serves for the abscissa of times; while the perpendicular distances from the curve drawn by the mirror attached to the needle of the galvanometer to this axis of times give the ordinates of the curve drawn by the latter. Rapid printing-paper was used, and an ordinary gas-flame gave a sufficiently strong spot of light to produce an actinic effect.

Fig. 1 represents the action of a modification of Trouvé's battery. During this experiment, which lasted for thirty minutes, there were five ohms in the external circuit. The right-hand portion of

Fig. 1.

Fig. 2.



the diagram shows the strength of current when the circuit was made, and it will be observed that the battery was not at its best

until ten or twelve minutes after making the circuit; from this maximum point the strength of the current gradually diminishes. Fig. 2 shows the action of the same battery with ten ohms external resistance. Under these conditions we find at the instant of making circuit a strong current, which rapidly diminishes within the first five minutes to one sixth of its first strength.

Knowing the distance of the galvanometer from the sensitive paper, the strength of the current may be calculated by measuring the distance between the two lines at any instant, and proceeding as with an ordinary galvanometer and scale.

From a comparison of the two figures the electromotive force may be determined by Ohm's law, if the distance between the lines is measured at the instant the current is made. Then knowing the electromotive force, current, and external resistance, we can readily find the internal resistance. This resistance will be the liquid resistance of the cell only for the moment that the circuit is made, for afterwards the variation in electromotive force due to polarization, and the change in resistance of the liquid due to electrolytic action will combine to cause changes. Since, however, the changes in electromotive force due to polarization are much more rapid in their action than the changes in battery resistance, a very small error will be introduced if we compare points near each other on those parts of the curve in which the variation in current is greatest: during the small fraction of a minute that is taken, the change in battery-resistance will be infinitesimal and may be neglected.

We have selected these photographs as an example of the large variations that some batteries present, and the consequent usefulness of some such way of studying their action. From measurements upon these photographic charts the variations in electromotive force and internal resistance can be studied by obtaining such charts under different conditions of external resistance. It is evident that the same photographic method can be employed to study the swing of the needle of a short coil-galvanometer, which indicates the gradual heating of a thermopile. In this way the conduction of heat along a bar could be studied.—Silliman's *American Journal*, May 1885.

Jefferson Physical Laboratory.

ON AN INSTRUMENT RESEMBLING THE SEXTANT, BY WHICH ANGLES WITH THE HORIZON CAN BE MEASURED. BY E. H. AMAGAT.

Imagine a sextant of which the optical axis of the telescope, instead of being oblique in reference to the fixed mirror, which I shall call B, is perpendicular to it. Arrange the movable mirror A so that its axis of rotation coincides with one of its edges, and cuts the optical axis. Suppose that the mirror B, instead of being fixed, is movable about an axis parallel to the plane of the graduated circle, and perpendicular to the optical axis; in these conditions,

the image twice reflected from any given point will be displaced in a plane perpendicular to that of the circle, when B turns about its axis. If the circle is placed horizontally, all the objects on the same vertical plane will be seen to pass in succession.

Now let us suppress the telescope, and replace it by a simple vertical slit placed between the eye and the axis of rotation of A; suppose, moreover, the mirror is silvered all over. By placing the eye at a convenient height in front of the slit, we can look directly at an object above or below the mirror (according to its height), and we can make it coincide with the image twice reflected of another object which may be at any height, since the movement of B may bring it vertically to the desired height. A reading made as with the sextant will give at once the angle of the two objects projected on the horizon, such as would be obtained with a graphometer.

It follows from the preceding that the coincidence should be exact with the image of the axis of rotation or vertical edge of A, which would reduce to zero the field of the objects seen in B by double reflection; but it is easy to see that by slightly inclining the visual ray so as to have a sufficient field, the error produced will be extremely small from the properties of minima, since we move but little from the position in which the error is completely nullified; with a small instrument which is only 7 centimetres in the side, an operator who is but little practised may take, in a few seconds, angles with sufficient accuracy for most ordinary operations; the approximation mainly depends on the horizontality of the instruments. Although the coincidence becomes more difficult and the exactitude less for very large angles, we may work directly up to 140° .—*Comptes Rendus*, April 27, 1885.

ON THE PRODUCTION OF ALTERNATING CURRENTS BY MEANS OF
A DIRECT-CURRENT DYNAMO-ELECTRIC MACHINE. BY JOHN
TROWBRIDGE AND HAMMOND VINTON HAYES.

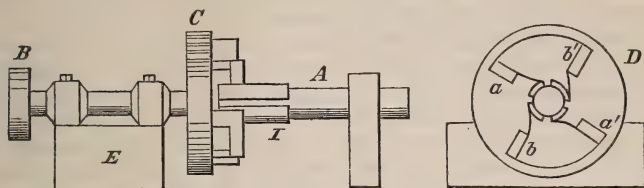
It is often desirable to transform a direct current into an alternating one for the purpose of obtaining electricity of high tension by means of a Ruhmkorff coil, for studying the effects of stratifications in vacuum tubes, or for employing alternating currents in the study of magnetism. The best way is undoubtedly to employ an alternating dynamo-electric machine, as has been done by Spottiswoode. When, however, only a direct-current machine is available, the following method can be employed:—

The dynamo machine, if it is not a shunt-wound machine, is shunted by a suitable resistance. We have employed for this purpose thin ribbon-steel about 1.5 centim. broad and .01 millim. in thickness. The remaining portion of the current from the machine is conducted to two brass or copper segments a, a' , fig. 2. This current is led to the primary coil, for instance, of a Ruhmkorff coil from two other segments b, b' . These segments are fixed upon a

cylindrical shaft A (fig. 1), which is stationary. A belt passing over the pulley B turns the wheel C, upon the face D of which

Fig. 1.

Fig. 2.



revolve four brushes which connect the adjoining segments. The brushes *a a*, *b b* are made adjustable, the two adjoining brushes being electrically connected, and a small stream of water plays upon the segments of the commutator. The character of the spark produced by a Ruhmkorff coil which is marked by alternating currents has been studied by Spottiswoode. Without condensers in the secondary circuit, a bright yellow glow spans the distance between the two terminals of the coil, which partakes more of the character of a voltaic arc than of the ordinary discharge from a Ruhmkorff coil. The apparatus which we used produced three thousand reversals a minute. This rate was too rapid for the best effects with a Ruhmkorff coil. It enabled us, however, to study the musical note produced in the cores of the electromagnet by rapid reversals of the current in the electromagnet, and also the heating effects which have been so often studied.—Silliman's *American Journal*, May 1885.

Jefferson Physical Laboratory.

THERMO-ELECTRO-PHOTO-BARIC UNIT.

BY PLINY EARLE CHASE, LL.D.

The earliest attempt at measurement, with a view to demonstrate the correlation of thermal and electric energies, appears to have been that of Principal Forbes, who found, in 1832 (*Phil. Mag.* iv. p. 27), that the conductivity of metals for heat and electricity is nearly the same. The dimensions of absolute measure involved were *M*, *L*.

In 1843 Joule published his discussion of the calorific effects of magneto-electricity, and his determination of the mechanical equivalent of heat (*Phil. Mag.* xxiii. pp. 263, 347, 435), using the same dimensions, *M*, *L*.

In 1856 Weber extended the correlation of Forbes (*Pogg. Ann.* xcix.), by showing the approximate equality of the electro-magnetic ratio to the velocity of light ($L T^{-1}$).

Wolf's discovery of the sun-spot period was followed, in 1857, by the investigations of Lamont and Sabine, showing the identity

of the sun-spot periods with the periods of magnetic perturbation (Mag. and Meteorol. Obs., Toronto, III. lxviii.; St. Helena, II. cxxi.-cxxxvi.).

In 1860 Henshall showed the influence upon sun-spots which is produced by Mercury, Venus, and Jupiter, when in conjunction with the same face of the Sun (*Cosmos*, xvii. p. 573).

In 1863 Chase showed (Proc. Amer. Phil. Soc. ix. pp. 283-288; Phil. Mag. xxviii. pp. 55-59) that the mass of the Sun can be approximately estimated from the influence upon the barometer of the constrained "relative motions" of the Earth and Sun. In 1864 he showed, by the investigation which received the Magellanic medal (Trans. Amer. Phil. Soc. xiii. pp. 117-136; Proc. Amer. Phil. Soc. ix. pp. 425-430; Phil. Mag. xxx. pp. 52-57), that the magnetic disturbances of the Sun and Moon are many times greater than simple tidal disturbances, and that they can be very closely represented by the disturbances of gravitating pressure, under constrained and "coercitive" relative motion. In 1869 he further showed (Proc. Amer. Phil. Soc. xi. pp. 103-107) that the constrained relative motion at Sun's surface represents a cyclical gravitating and electric disturbance which acts with the velocity of light.

In 1873 Maxwell (Electricity and Magnetism) published his theory that light consists of a disturbance in a medium susceptible of dielectric polarization.

In 1884 Langley (Researches on Solar Heat) confirmed the identity of thermal, electric, and luminous radiation, for which Chase had suggested probable reasons in 1864 (Proc. Amer. Phil. Soc. ix. p. 408), and Draper in 1872 (Phil. Mag. xvi. pp. 104-107).

All of the foregoing investigations can be coordinated, in the region of greatest known energy, by means of the kinetic unit $\frac{\mu v_0^2}{2}$, in which μ represents an infinitesimal particle, and v_0 is the velocity of light, the electric ratio, the projectile velocity which represents thermo-dynamic energy at Sun's surface, and the projectile velocity which represents the maximum energy both of constrained rotation and of free revolution in the solar system.

The greatest constant energy of free revolution, which can be given to μ by solar attraction, is $\frac{\mu_k v_k^2}{2}$, v_k being the velocity of circular orbital revolution at the Kantian radius r , where solar rotation and orbital revolution are synchronous. The same energy would give synchronous radial or elliptic revolution through or about a major axis $2r_k$, under gravitating acceleration varying inversely as r^2 .

The energy which would be required to produce synchronous radial oscillation, under the constant gravitating acceleration g_k , would be π^2 times as great, or $\frac{\mu \pi^2 v_k^2}{2}$.

The energy which would be required to produce constant radial oscillation in the region of maximum solar gravitation and coercitive

force (at Sun's surface) would be k^4 times as great, or

$$\frac{\mu \pi^2 k^4 v_k^2}{2} = \frac{\mu v_0^2}{2},$$

k being the ratio of the Kantian radius to the solar radius.

The time which would be required to communicate this maximum energy is t_k , the time of virtual projection against uniform resistance, in the region of greatest solar energy, which is also the time of solar half-rotation, as well as the minimum time of synchronous elliptic, circular, and radial oscillation in the solar system.

The ordinary thermal and gravitating units may be deduced from the general unit by means of the equations

$$g_n = \frac{m_n r_0^2 v_0}{m_0 r_n^2 t_k}$$

$$T = \frac{v_0^2}{2g_3 h}.$$

In the second of these equations T represents the mass of water which could be heated one degree by μ of oscillating luminiferous æther, or the number of degrees to which μ of water could be heated; g_3 , gravitating acceleration at Earth's equator; h , the linear dimension of the mechanical unit of heat.

The harmonic values are as follows:—

$$m_0 = 329414 m_3,$$

$$r_0 = 198.923 r_3,$$

$$g_0 = 27.765 g_3,$$

$$v_0 = 185500 \text{ miles per second,}$$

$$g_3 = 32.033 \text{ feet per second,}$$

$$T = 10,775,492,000,000^\circ \text{ C.}—\text{Communicated by the Author,}$$

having been read before the American Philosophical Society, April 17, 1885.

THE CHASE-MAXWELL RATIO. BY PLINY EARLE CHASE, LL.D.

In 1872 (Proc. Amer. Phil. Soc. xii. p. 394) Chase showed that the tendency of particles, in exploded gases, toward primary and secondary centres of oscillation leads to a permanent *vis viva* of equilibrium which is $\frac{5}{9}$ of the *vis viva* of explosive projection, and that the synchronous action of the Sun and the Earth upon the oscillating particles furnishes a ready method for estimating the Sun's mass and distance. He also showed (ibid. pp. 403-405) that the successive planetary positions in the solar system illustrate the influence of æthereal oscillations of a similar character. In 1875 he showed (*op. cit.* xiv. p. 651) that the mean velocity of expanding gaseous pressure is $\frac{2}{\pi}$ of the corresponding constant velocity of revolution; the ratio of *vis viva*

is therefore $\frac{4}{\pi^2} = .405285$, and we have

$$K : k :: 1.405285 : 1.$$

In 1877, Preston (Phil. Mag. iii. p. 453; iv. p. 209) showed "that a physical relation exists between the velocity of the particles of a medium constituted according to the kinetic theory, and the velocity of propagation of a wave in the medium." Maxwell calculated the numerical value of this relation at $\sqrt{\frac{5}{9}}$, which represents Chase's ratio of relative *vis viva*; but he did not give the method by which he reached that result, and no record of it was found among his papers. The following thermodynamic demonstration may therefore be satisfactory to those who have found any difficulty in accepting the more simple and more general photodynamic proof which is furnished by reference to oscillatory centres.

If we represent the density of a gas, $\frac{nm}{v}$, by ρ , the fundamental equation of pressure becomes

$$p_{\mu} = \frac{\rho c^2}{3} = \frac{p_0}{3}. \quad \dots\dots\dots (1)$$

Alexander Naumann (*Ann. Pharm.* 1867, pp. 142, 267; *J. B.* 1867, p. 62) showed that

$$\mu = \frac{3}{2}(\gamma' - \gamma), \quad \dots\dots\dots (2)$$

μ being the heat of molecular motion, or mean *vis viva* of a perfect gas; γ' , the specific heat under constant pressure; γ , the specific heat under constant volume; $\gamma' - \gamma$, the heat of expansion, or *vis viva* of mean velocity. The total specific heat is therefore

$$\theta = \mu + \gamma' - \gamma = \frac{5}{3}\mu. \quad \dots\dots\dots (3)$$

Hence $p_{\theta} : p_0 :: 5 : 9, \quad \dots\dots\dots (4)$

$$v_{\theta} : v_0 :: \sqrt{5} : 3. \quad \dots\dots\dots (5)$$

Prof. d'Auria, in a special investigation relating to the dynamics of direct-acting pumping-engines, not yet published, has found, by analogy, that

$$\mu = \frac{6\gamma}{\pi^2} = .607927\gamma. \quad \dots\dots\dots (6)$$

Substituting this value in eq. (2) we get Chase's result:—

$$\gamma' - \gamma = .405285\gamma \quad \dots\dots\dots (7)$$

$$\gamma' = 1.405285\gamma, \quad \dots\dots\dots (8)$$

The exactness of agreement between this *à priori* value and the one which was found by Röntgen (1.4053; *Pogg. Ann.* 1873, pp. 148, 603) is very remarkable.—*Communicated by the Author, having been read before the American Philosophical Society, April 17, 1885.*

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END OF THE NINETEENTH VOLUME.

Fig. 5.

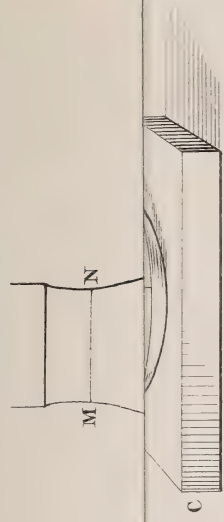


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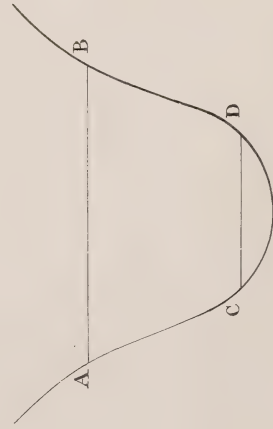
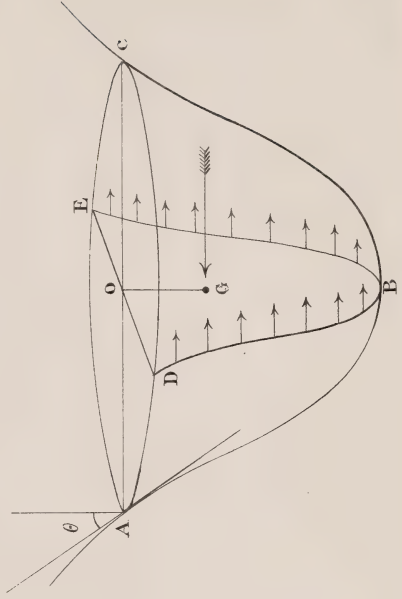


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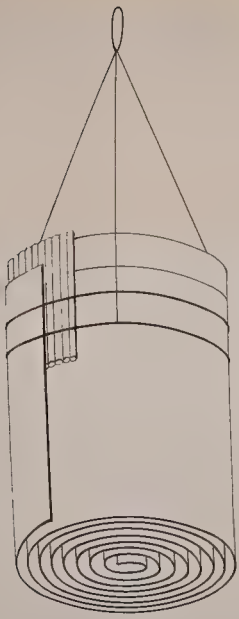


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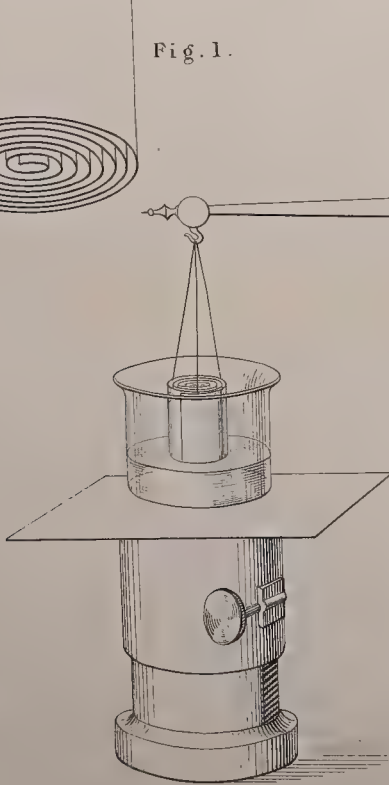


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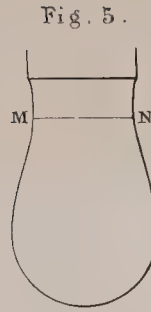


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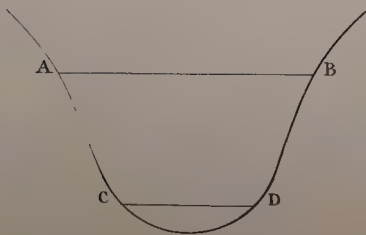


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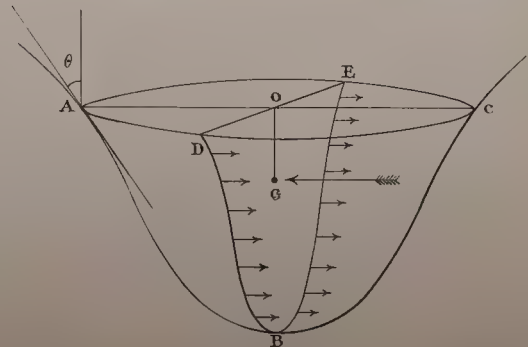


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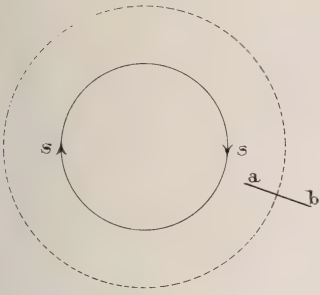


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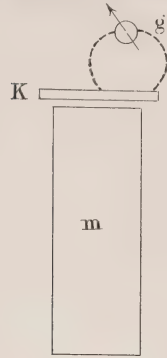


Fig. 2.

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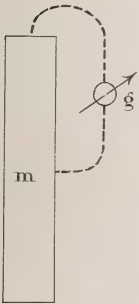


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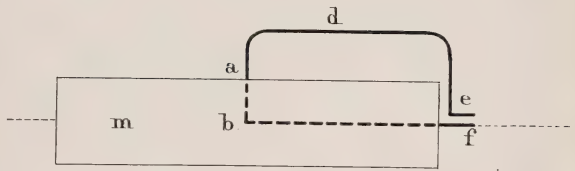


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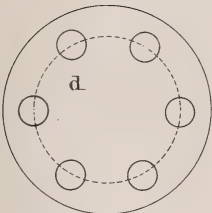
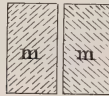


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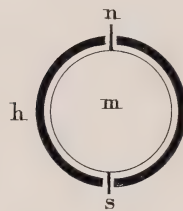


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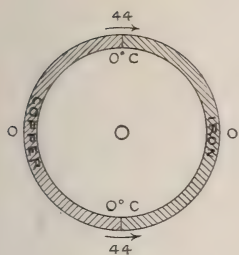


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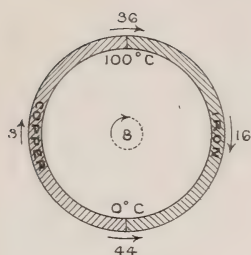


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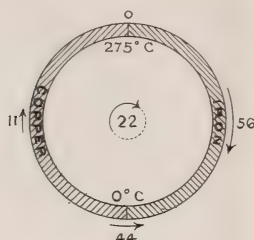


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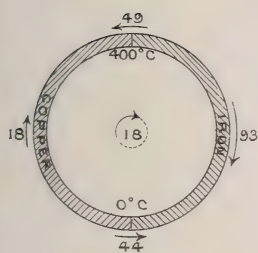


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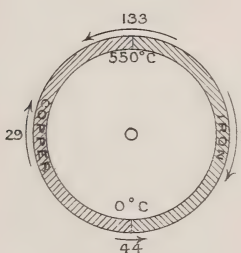


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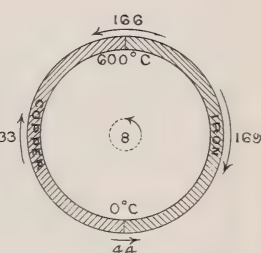


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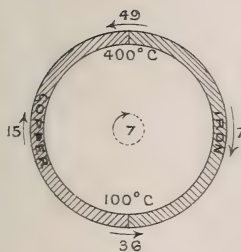


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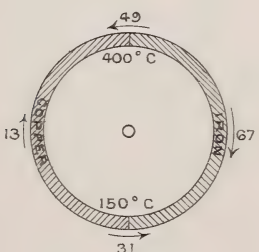


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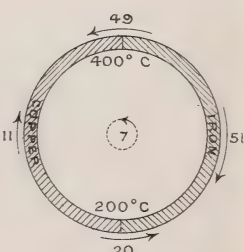


Fig. 9.

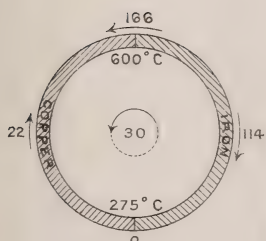


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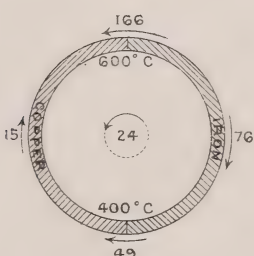


Fig. 11.

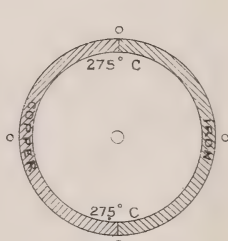
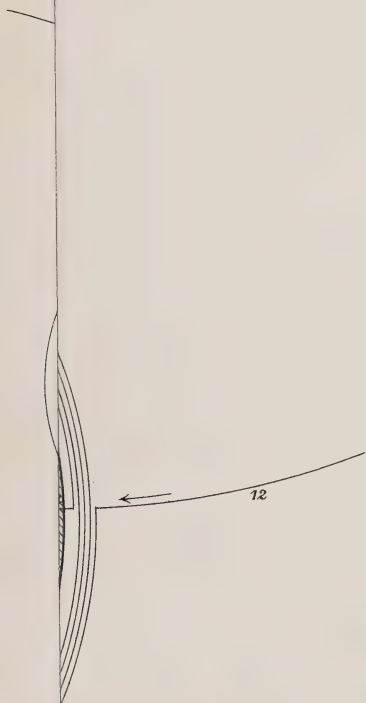
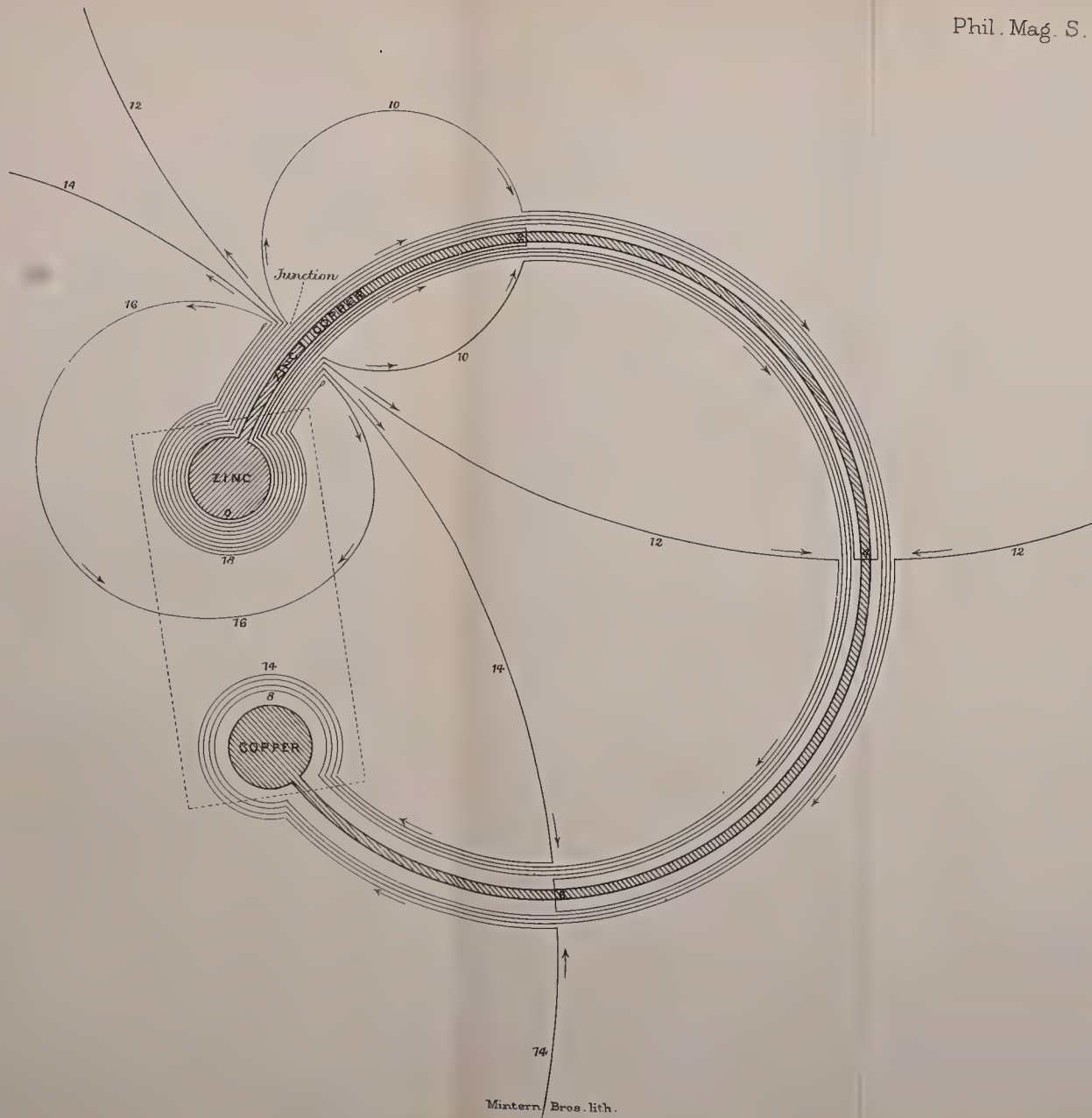
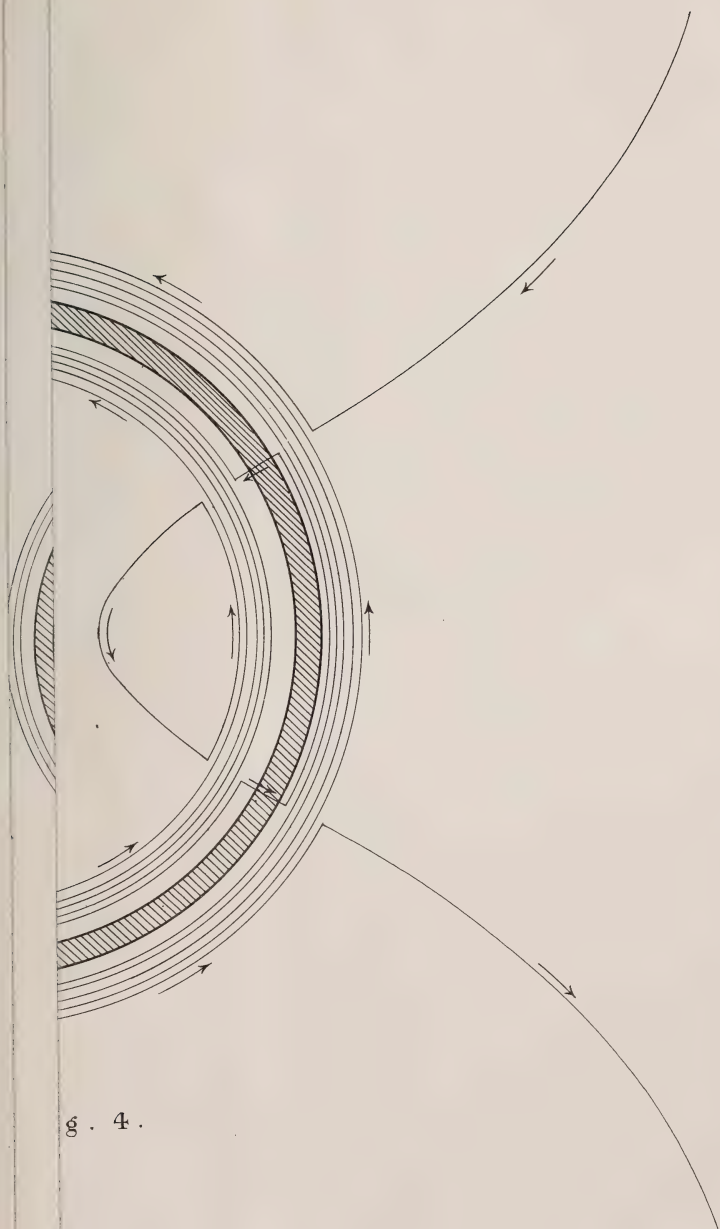


Fig. 12.







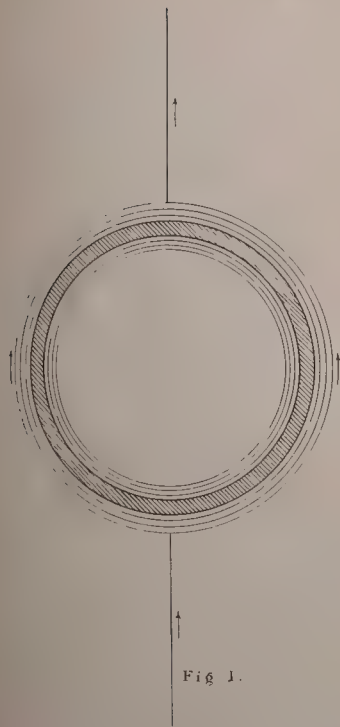


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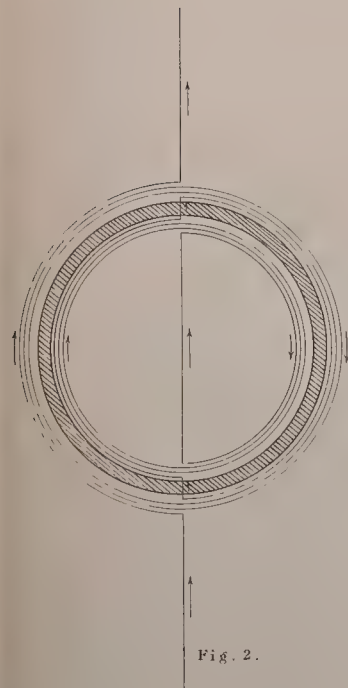


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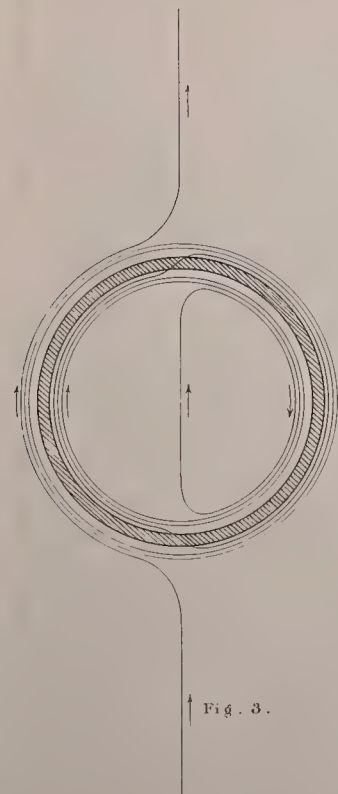


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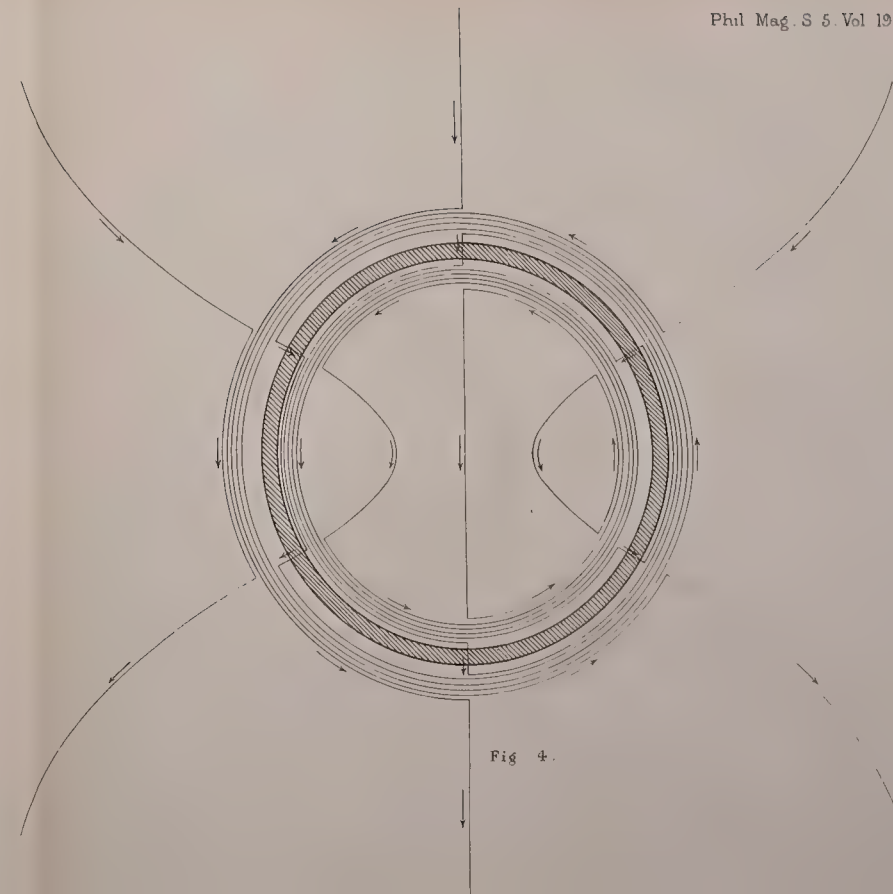


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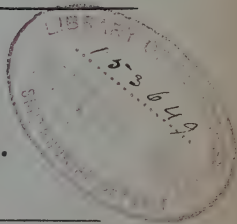
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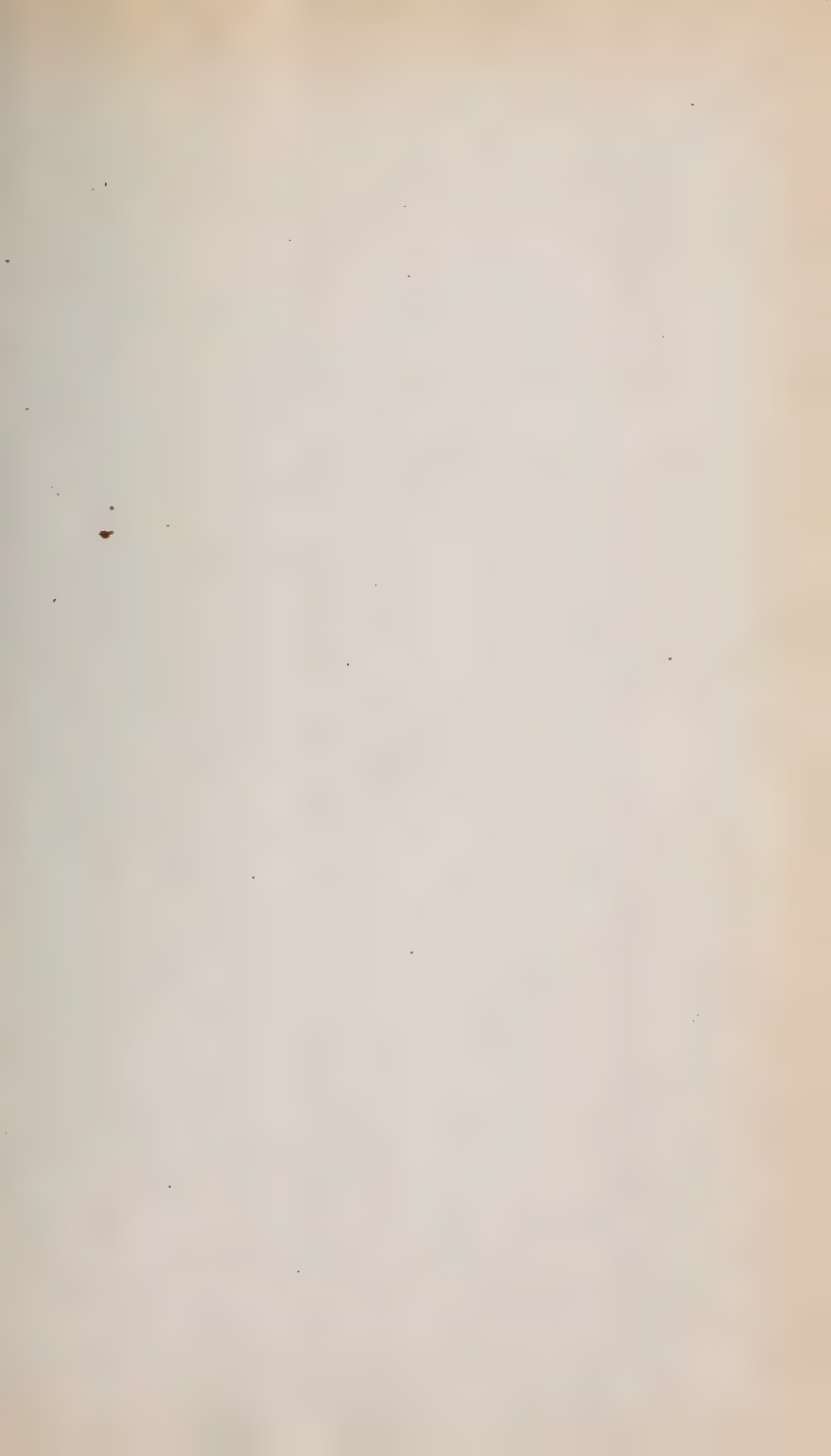
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